Toward the Inclusion of Fishery Interactions in Management Advice

Proceedings of a Workshop at the Bedford Institute of Oceanography (October 30 - November 1, 1984)

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edited by

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ABSTRACT

Mahon, R. 1985 [ed.]. Towards the Inclusion of Fishery Interactions in Management Advice. Proceedings of a Workshop at the Bedford Institute of Oceanography (October 30 - November 1, 1984). Can. Tech. Rept. Fish. Aquat. Sci. No. 1347.

This report contains the proceedings and contributed papers of a workshop on the incorporation of fishery interactions into management advice. The main topics were: technological or by-catch interactions; identification of fishery systems for modelling and application of existing techniques; and the Scotian Shelf as a system. Conclusions were: that immediate steps could be taken to explore the feasibility of accounting for technological interactions in management; that large scale models such as the ICES multispecies virtual population analysis were at present neither feasible in the Canadian Atlantic, due to resource limitations, nor appropriate, due to theoretical difficulties; that incorporation of species interactions could only be initiated by considering small systems of species with possible key interactions e.g. cod-capelin, silver hake-haddock. In general, dissociation of biological from socioeconomic advice, and of these from the allocation process were considered to be problem areas in moving towards a more inclusive approach to fishery management.

RÉSUMÉ

Mahon, R. 1985 [ed.]. Towards the Inclusion of Fishery Interactions in Management Advice. Compte-rendu d'un atelier qui s'est déroulé à l'Institut Océanographique de Bedford (30 octobre - 1er novembre 1984). Can. Tech. Rept. Fish. Aquat. Sci. No. 1347.

Le présent rapport contient le compte-rendu d'un atelier sur l'incorporation des interactions halieutiques dans les conseils relatifs à la gestion, ainsi que les exposés présentés à cette occasion. Les principales questions traitées ont été les suivantes : interactions technologiques ou des pêches accessoires; identification des systèmes halieutiques pour l'élaboration de modèles et l'application des techniques existantes; le plateau Scotian considéré comme un système. Les conclusions suivantes ont été tirées : on peut prendre immédiatement des mesures pour déterminer s'il est faisable de rendre compte des interactions technologiques dans la gestion; les modèles à grande échelle comme l'analyse de la population virtuelle multispécifique de CIEM ne sont à l'heure actuelle ni applicables dans la région canadienne de l'Atlantique, à cause des ressources limitées, ni appropriées, à cause de certaines difficultés théoriques; L'incorporation des interactions touchant les espèces comportant des interactions clés, par exemple, morue-capelan, merlu argenté-aiglefin. En général, on considère que le fait de dissocier le conseil biologique de conseil socio-écomonique et le fait de dissocier ces derniers du processus de répartition constituent des zones de problèmes qui gênent la marche vers une approache plue complète à la gestion de la pêche.

PREFACE

The Marine Environment and Ecosystem Subcommittee (MEES) is one of seven subcommittees of the Canadian Atlantic Scientific Advisory Committee (CAFSAC). The roles of the others (e.g. Groundfish, Invertebrates and Marine Plants) are more clearly defined inasmuch as they usually provide advice which has been requested by the fishery managers.

The role of MEES has been different, its terms of reference are:

- consider the effects of environmental changes, ecosystem interactions, or human interventions on Canadian Atlantic fisheries and provide the scientific basis for CAFSAC advice; and

- provide a forum for interdisciplinary discussion of ecosystems and environmental topics in relation to Canadian Atlantic fisheries.

Previous topics reviewed by MEES have included the effects of the Canso causeway, the possible impacts of offshore oil and gas development on fisheries, and current knowledge of the Scotian Shelf Ecosystem. There was a lull in the activities of MEES following the Scotian Shelf workshop in fall 1981. Indeed, there was some question about whether MEES could play a useful management role. When the subcommittee met again in mid 1983 there was concensus that to play a useful role MEES should address more specific practical matters which could feed directly into the existing system for management of fisheries and fishery ecosystems. To achieve this it was agreed that MEES should attempt to function more in the manner of the fishery specific subcommittees. The members identified "multispecies" problems as being the first broad area of concern and this workshop was planned. The stated objectives were:

a) review and evaluate the methods of resolving problems due to interactions in fisheries;

b) formulate recommendations to CAFSAC regarding the incorporation of systems related advice into the advisory process; and

c) formulate recommendations on management based on the workshop results.

These objectives were deliberately broad in the expectation that this workshop would lead MEES into more specific areas in which to develop an ongoing process for including fishery interactions and environmental influences in management advice.

WORKSHOP REPORT

SESSION I: HISTORICAL OVERVIEW

CHAIRMAN: R. Mahon
RAPPORTEUR: S. Campana

The incorporation of multispecies considerations into the management advisory process has been a matter of growing concern to fishery managers and researchers over the last decade (Mercer 1982). Owing to the complexity of marine fishery ecosystems, and the difficulty of studying them, there have been very few cases in which research has progressed to the point where the results can be applied. Furthermore, when these results do become available and are formulated as advice, it is not at all clear that existing management structures will be able to accomodate the advice.

The broad objective of this workshop is to identify the areas of fishery management where the most benefit could result from taking fishery interactions into account, and to advise on the appropriate route to take in doing so. More specifically the objectives were defined as: review and evaluate the methods of resolving problems due to interactions in fisheries; formulate recommendations to CAFSAC regarding the incorportion of systems related advice into the advisory process and; formulate recommendations on management based on the workshop results. In fact it was anticipated that realization of these objectives would be an ongoing function of the membership of the Marine Environment and Ecosystem Subcommittee of CAFSAC, and that this workshop would be a starting point.

The remainder of this session was comprised of two review papers. The first, by R. Halliday and A. Pinhorn, gave an overview of present management strategies. The paper indicated that in a limited number of cases some account has been taken of species interactions. In the case of cod and capelin this simply takes the form of setting low exploitation rates for capelin in recognition of its importance as forage for cod. A more quantitative attempt was tried in the case of 4T cod where the effect of mackerel biomass on cod recruitment was modelled. The model failed to be predictive and its use was discontinued after two years. The general conclusion of the paper is that the recent trends in groundfish abundance and catch rates have been consistent with the overall stated objectives of management, whereas those in pelagic fish stocks have not. Squid fisheries are in general too recent for conclusion. The point is made that failure to achieve management objectives does not necessarily mean that the strategy and models applied were inadequate. Especially, it does not mean that they were inadequate because they failed to include species interactions. This remains to be demonstrated.

In the discussion the point was made that managers have resisted more complex, e.g. multispecies, approaches. If this attitude is to be changed, the next generation of multispecies models must be mathematically correct, intuitively sound and provide a clear advantage over unispecies assessments. This job may be easier now than in the past. The unispecies models that have been successful in rebuilding the Scotian Shelf stocks from record low levels, may be less successful now that the stocks are closer to equilibrium levels.

In the second paper, by J. Anderson and G. Lilly, interactions reported for the Newfoundland Region (NAFO Subdivisions 2 and 3) were reviewed to determine the types that have occurred and the extent to which they have posed management problems. Most interactions were of two types - biological, involving predation, or technological, involving species by-catch. Predation was further subdivided - the effects of prey on predators and the effects of predators on prey. The possible influence of reduced capelin abundance on predators (principally Atlantic cod, Atlantic salmon, seals, puffins) was clearly the area of most concern. Although there are indications that capelin abundance affects growth (cod), natality (puffins) and feeding migrations (cod, whales), the data are inconclusive. Studies of how predation on pre-recruits affects recruitment have also been inconclusive. No clear statements regarding significant effects of predator/prey dynamics on the fishery have emerged. What is clear is that to answer such questions specific studies must be carried out.

Technological interactions are a recurrent management problem. The concern has been mostly: do by-catches of one species in a directed fishery for another represent a significant source of fishing mortality? Some of these have been deemed as having important effects whereas most have not. By-catch problems were gear not specific but most often occurred with changing fishing strategies involving an expanding fishery or the introduction of new gear types. Information on species distributions is important as the degree of overlap often determines when and where by-catches will be high.

These studies understate the importance of problems in data quality and quantity which is usually indequate for clear conclusions. There is a lack of data collected specifically to answer questions of the significance of by-catch. As with biological interactions, few specific studies have been set up.

SESSION II: HARVESTING INTERACTIONS

CHAIRMAN: W.B. Brodie
RAPPORTEUR: D.B. Atkinson

This session dealt with topics pertaining to the harvesting of multispecies fishery resources. Papers presented covered such areas as interpretation and analysis of multispecies fishery data, case studies on the Scotian Shelf, and implications of multispecies management on Canada's east coast fisheries. There were four presentations each followed by discussion.

W.B. Brodie reviewed methods and data used in analyzing multispecies fisheries interactions including interpretation of catch data from commercial fisheries and their use in models such as general production and linear programming. Research vessel survey data as well as observer data were also considered. Discussion centered on the limitations of by-catch data and the problems in interpreting what actually constituted the main-species. The possible use of linear programming techniques to establish a two-tier quota system was pointed out, as was the sensitivity of these types of analyses to changes in historic by-catch patterns.

Next, D.E. Waldron and A.F. Sinclair presented an analysis of by-catches observed in the Scotian Shelf foreign fishery and discussed their impact on domestic fisheries specifically the by-catch of major commercial groundfish species (cod, haddock, pollock) in the small mesh gear fisheries for silver hake and squid. Geographic distribution of by-catches supported the placement of the small mesh gear line. The paper showed that low by-catch levels of the major groundfish species are due to enforcement of the existing regulations and that cessation of the foreign fisheries would provide no appreciable increase in domestic yield of these species.

The question of changing mesh sizes in relation to the silver hake population was discussed. It was indicated that a significant increase in mesh size would bring about an increase in the effort required to catch their allocations. The amount of effort required would be unattainable. As a result, silver hake biomass would increase, with the possibility of a detrimental impact on other species. The restriction in area fished plus a reduction of effort in the squid and silver hake fisheries in recent years was an important factor in the low by-catch levels of groundfish.

A linear programming analysis of Scotian Shelf offshore fisheries by A.F. Sinclair used catch and effort data collected by DFO fisheries observers from Canadian TC 4 & 5 otter trawlers to evaluate technological interactions within this fleet. The fisheries were defined using cluster analysis. The linear programming solutions indicated that, except for 4X cod, all quotas for vessels greater than 100', could be taken together. The solutions obtained under the objectives of maximising total catch or total revenue were identical. However, certain "money losing" fisheries were dropped from the optimal solutions under the objective of maximising net revenue. The predicted effort distributions differed greatly from those observed in 1982 and 1983. The addition of further constraints to the models may produce more realistic solutions.

The discussion noted that the optimal solutions proposed by the linear programming model indicated that the fleet did not appear to be following optimal patterns. This was at least partially due to the fact that, in reality, the fleet never takes all of each quota. Redfish and flatfish quotas are usually undercaught. It was further noted that the economic terms used in the analysis were highly simplified and did not necessarily represent real conditions.

Finally, R. O'Boyle discussed the implications of multispecies principles for Canada's east coast fisheries management. Before effective incorporation of multispecies management principles could occur, some changes relating to the division of labor between scientists and managers should be considered. The move is viewed as desirable, but changes in the existing structure should be practical as well as theoretically sound. Technological interactions can be addressed at present, but biological interactions pose a much greater problem.

The discussion of this paper raised the point that these interactions should first be considered at levels other than biological, such as industry problems, fleet interactions etc. There was some discussion of the concept of system management, and how man's involvement in various systems could be compared with predator-prey interactions.

SESSION III: METHODS FOR IDENTIFICATION OF MULTISPECIES SYSTEMS

CHAIRMAN: W. Silvert
RAPPORTEUR: S. Murawski

Five presentations concerning the problem of multispecies system identification were made during the session. The first four (Schwinghamer, Murawski, Rice, and Myers) were methodological, describing techniques that have been applied to system identification problems in marine ecology. Silvert reviewed the process of hypothesis generation and testing and commented on some of the fundamental problems of model identification and validation in light of the institutional hesitancy to utilise results of models that differ from current practices in the fishery management process. This hesitancy is understandable since it has previously been frustratingly difficult to construct multispecies fishery models and verify them empirically as a basis for future fishery management decisions.

P. Schwinghamer described the use of path analysis (causal analysis) as a basis for identifying significant relationships among species and environmental variables in ecosystems under study. Path analysis "provides a method of assessing the validity of causal interpretation by comparison of correlations among variables calculated from the resulting path diagram with those observed in the data set". In this way it is possible, by iteration, to select hypothetical models that are biologically acceptable and statistically most consistent with the data". The technique involves the construction of a structural map "path diagram" among the components of an ecosystem. The degree of correlation among various species/environmental variables is then assessed by applying regression techniques to the available ecological data. Finally, sets of regressors are chosen for particular models of the system. The adequacy of a particular model ("path") is "based upon choosing a parsimonious,

biologically sensible, and statistically significant regression model in which all of the regressors contribute positively to the overall significance of the equation". Schwinghamer described the application of path analysis techniques to model the relationships of exogeneous and endogenous variables in nearshore benthic ecosystems.

S. Murawski reviewed applications of cluster analysis to multispecies ecological data. The most prevalent use of clustering in the multispecies fishery context has been to identify species or site groups from bottom trawl survey data, or multispecies landings information. Other potential applications of cluster analysis for multispecies systems identification are to describe the grouping of species in relation to their environmental determinants of distribution (e.g., temperature and depth), to examine the organization of ichthyoplankton communities, and to elucidate feeding interrelationships among species by clustering dietary data. Murawski cautioned that although clustering may be useful for reducing the dimensions of multispecies data sets to a comprehensible level, results must be cautiously interpreted, may be ambiguous, and are not an adequate substitute for a mechanistic understanding of system organization.

J. Rice examined various ordination techniques employed to analyze multivariate data. His presentation was hierarchical, emphasizing that various techniques are applicable to different situations depending on the nature of multivariable data available, and the research question posed. Reasons for cautious use of discriminant functions analysis were presented. For true ordinations, direct gradient analyses, multidimensional scaling, correspondence analysis, and principal component analysis, among other techniques, are useful for reducing the dimensions of data sets, displaying relationship among taxa and sites and for analysing relationships among species and environmental variables.

R. Myers compared the use of traditional correlation-type analyses and time-series (ARIMA) models for describing the relationship of two or more variables that are measured in a time sequence. His presentation emphasized that the application of regression-type analyses to such variables (e.g., water temperature and salmon abundance) monitored over time can lead to spurious conclusions regarding causal mechanisms. Time-series techniques (i.e., Box-Jenkins analyses) allow for the examination of the dependancy of two variables measured in a time sequence, and for statistical modeling and prediction based on the relationship.

W. Silvert concluded the session with a discussion of model uniqueness and a summary of the previous presentations in the session. Silvert emphasized that ecosystems are inherently difficult to model because fundamental ecological relationships are poorly defined, in fact very few alternative hypotheses of how systems are organized and react exist. Since several alternative hypotheses may describe available empirical data gathered from an ecosystem, ecologists and managers are faced with the problem of model uniqueness. Only through an iterative approach to model identification-testing and verification can we eliminate invalid hypotheses and avoid ambiguities. However, fishery managers are notably unsympathetic toward the inability of scientists to discern between conflicting hypotheses, and they are unlikely to undertake experimental management in order to test hypotheses. Silvert proposed that given this institutional hesitancy we have to develop strategies for using suites of models in the management process rather than eliminating hypotheses one-by-one.

SESSION IV: THE SCOTIAN SHELF EXPERIENCE

CHAIRMAN: R. O'Boyle
RAPPORTEURS: J. Neilson and R. Stevenson

This session concentrated on the Scotian Shelf to see if and how multispecies principles could be applied there. The first group of papers in the session examined the biological, environmental and fishery characteristics of the Scotian Shelf.

The first two papers by R. Mahon and E. Sandeman explored the distribution of groundfish assemblages through cluster analysis. Breaks in species distributions were consistently identified at the tail of the Grand Banks, in the central Scotian Shelf and near Cape Cod. These papers suggested that the assemblages corresponded closely to known environmental patterns. On the Scotian Shelf the spatial patterns of species distribution were persistent in each summer from 1970 to 1981. Similarly, there were recurrent groups of species. This spatio-temporal persistence was considered to be potentially useful in managing technological interactions. However, the patterns could not be readily extended to the trophically based assemblage region/assemblage production unit approach described by Tyler and co-workers in the St. John's workshop on "Multispecies Approaches to Management Advice".

During the subsequent discussion, the point was made that biological groups of species, or areas defined by cluster analysis may not be useful for management. Other factors, such as fishing effort need to be considered. However, it was pointed out that the existing management boundaries are in broad agreement with the analysis. Further comment was made on the utility of using presence/absence data rather than abundance, and on other methodological aspects.

R. Trites and R. Loucks presented a brief review of Scotian Shelf oceanography. Since there is evidence that large scale processes play an important role in defining local conditions on the Scotian Shelf, they went on to examine large scale patterns in sea surface temperature (SST) variability in the Northwest Atlantic. Using empirical orthogonal function (EOF) analysis they showed that the most important pattern in the data

is a relatively uniform increase (or decrease) in SST coherently on a large geographic scale. The second mode of the EOF, however, shows that the Grand Banks area varies with opposite phase to the mid Atlantic Bight area, with the Gulf of Maine positioned in the region of the swing over. They hypothesised that SSTs are influenced directly and indirectly by meteorological forcing. Winter winds consitute an important direct influence. The indirect pathways involve river runoff and subsequent altered stability of the coastal water on the one hand, and the broad ocean gyres and the meanders and rings of the Gulf Stream on the other.

There was some discussion on the nature of the second mode of the empirical orthogonal function analysis presented in this paper. As work is continuing nothing final could be specified. The significance of warm core eddies off the southern Scotian Shelf was highlighted. During this discussion it was pointed out that rings also occur off of the Southern Grand Banks. Future analyses of ecosystem structure in these areas should consider the effects of these rings.

T. Koslow presented the hypothesis that environmental effects, such as those suggested by Trites and Loucks, are the main determinants of recruitment variability in the Northwest Atlantic. Where biological interactions occur, their importance is minimal. The hypothesis is supported by patterns of recruitment observed for Northwest Atlantic fish stocks. Recruitment is highly correlated within species, particularly cod and haddock, over large spatial scales, suggesting that recruitment is driven by large scale environmental factors. Salinity and winds as well as sea surface temperature tend to be correlated over large spatial scales in the region. These physical variables explain a significant fraction of the variability in recruitment to most cod, haddock and herring stocks in the Northwest Atlantic.

Discussion following this paper was lively since virtually no biological feedbacks are admitted. There were opinions that recruitment failure in the early 1970s could have been due to recruitment overfishing rather than the environment, that possible stock-recruitment relationships should be considered more fully, and that the accuracy of certain VPA recruitment estimates is questionable; but the fact is that the latter are the best available.

Finally, A. Sinclair presented the fishery distribution on the Scotian Shelf as defined by species composition in the commercial catch. Consistent with Mahon and Sandeman's work, he showed a marked dichotomy between the species mix in the shallow waters of the eastern and western Scotian Shelf. Cod and flatfish dominated in the eastern area whereas haddock and pollock dominated in the west. In deeper areas the fishery is dominated by redfish. The cod, haddock, redfish and pollock fisheries were very clean with 80% of the catch being the target species. On the eastern shelf there was a mixed fishery for flatfish, and on Georges Bank one for cod, haddock and pollock.

The discussion of this paper centered on comparing these results to those presented by Mahon and Sandeman. In general, similar patterns emerge, considering that fishermen search for the highest catch rate. The ability of fishermen to make clean catches appears to be area specific.

In general these papers presented evidence for the dominant influence of environment on the Scotian Shelf system. Little in the way of biological interactions was suggested.

The second group of papers examine past, present and possible future modeling exercises for the Scotian Shelf. The first, by R. O'Boyle, reviewed the two tier approach of ICNAF (1974-77) which placed a second overall TAC as a limit to the stock specific ones. This was intended to account for biological and technological interactions. It was introduced into a difficult international forum and is not likely to be effective today: fewer, not more, regulations are required. Other TAC structures might be more appropriate but were not discussed.

The second paper, by R. O'Boyle, J. Rice, and J.J. Maguire reviewed the ICES approach explored in the 1984 Multispecies Workshop in Copenhagen. The model used multispecies VPA (MSVPA) which has been under development since 1979. However, it requires large amounts of data, and at present suffers from theoretical drawbacks. Even when these are worked out, it may not be useful in our area due to the dramatic difference in catch-age composition on both sides of the Atlantic. The main objective of MSVPA is to define age 0 and 1 predation mortalities for the industrial fisheries of the North Sea. There are no fisheries on these age groups in our waters. The single largest problem with the MSVPA at present is that it is age, rather than size structured. Although the Georges Bank model (George), which is size structured, could be used in lieu of MSVPA, much data on prey size and selectivity is lacking and 'George' has been shelved temporarily. Regarding MSVPA, tuning the analysis is still an unaddressed question.

The last paper, by J. McGlade and P. Allen, looked at the future in terms of treating the fishery as a complex system. One of the most important points, which they outline, is the fact that ecosystem is probably the most stable point of the overall system, in terms of identifiable cycles, and that many of the perturbations observed in contemporary fisheries are the result of human intervention, both in the demands placed on the system itself, and the policies used to govern that activity. By accepting the concepts of evolution, complexity and the role of fluctuations at critical points in the system, McGlade and Allen suggest that it is possible to identify the evolutionary trajectories that are in place, and further predict areas where perturbations are most likely to shift one component on to an alternative trajectory, much like moving through the branches of a tree. Moreover, they stress the importance of examining the impacts of management decisions that are made totally outwith the ecosystem, but which may have many unforseen implications in terms of future

resources. They also show the problems that ensue when the stochastic effects of fluctuating parameters, such as birth rate, and natural and fishing mortality rates, are ignored, and conclude by asking questions about the fundamental role of fisheries mangement.

The highlights of the day's talks reduce to (1) the similarity between the patterns of fish species distribution and the physical environment on the East Coast, (2) the influence of the environment on recruitment and distribution, and (3) our ability to define technological interactions but not biological ones. Papers presented thus far have shown that on theoretical grounds, maximization of the yield of all resources simultaneously was impossible. Certainly, multispecies considerations are needed. This led to the observation that one should refer to this next step as multisystem rather than multispecies management, as we are managing an industry not just a segment of it. The work of McGlade and Allen suggest that bifurcation theory may be appropriate. The system may not be reversible. Such an approach would also take a longer view of system dynamics that is now lacking.

Parameterization of complex system models is still in the future. The obvious question is -- what can we do now? Whereas, progress has been made in many aspects of multispecies research, papers presented in this session attest to this, application to management problems has seldom occurred. Fishery managers need to be educated regarding new approaches. One way to initiate the process of including multispecies principles into management advice would be to begin with well defined, technological interaction problems.

SESSION V: GENERAL DISCUSSION AND RECOMMENDATIONS

CHAIRMAN: R. Mahon

In order to focus the discussion, this final session was broken into three topic areas: past performance, technological interactions, and biological interactions. Specific recommendations are listed at the end.

a) Past performance

Whereas, the current management strategy and associated techniques have been successful in rebuilding many stocks, mainly groundfish, others have not recovered. Recent events suggest that as we approach historical stock levels and attempt to manage at equilibrium levels the methods will not be sufficiently precise to provide satisfactory results.

Although there was general agreement that technological and biological interactions could be causing some of the problems, it was felt that we still did not know whether the poor performance of models currently in use was due to: their being the wrong single species models, their being wrong because they were single species models; or inadequacies in the data being used as input. In the latter case refinement of data collection and model specification would be the appropriate routes and already subsume much of the time spent on assessing stocks. Ways of addressing the second of the above possible causes of difficulty are covered in the next two sections, but first there was area of general concern to participants.

This concern was about the role of the biologist (includes research scientist) in the assessment process. In general it appears that managers are not asking sufficiently specific questions or attempting to explore the implications of various management scenarios. Biologists cannot give answers to all possible questions, and must therefore anticipate management questions. One option would be for biologists to provide general models which the managers could manipulate to answer their own questions. Another would be to increase the degree of interaction between managers and biologists. The latter option was considered most desirable since management decisions frequently influence the kinds of, and quality of, data available to biologists. There was a general feeling that although biologists should not be directly responsible for management policy and decisions, they are presently too far removed from the process. There was agreement that a socio-economic advisory system parallel to CAFSAC could provide an appropriate forum for exchange with the biological advisory process (see Recommendation 5).

b) Technological interactions

It was felt that techniques for analysing technological interactions, and for providing advice on how to deal with them, were at a stage of development which warranted discussions on their trial application. Initiation of discussion with managers was felt to be particularly important since application of these techniques would require some adaptations in the allocation process (see Recommendation 2).

c) Biological interactions

At this meeting, no models of biological interactions which were developed to the point where they would be applied came forward. Furthermore it was clear that there has been no consistent forum in which to discuss the progress of such models and to expose them to the peer review process which characterizes single species assessments. The identification and ongoing review of a few likely, two or three species systems (key interactions) with potential management implications was seen as an appropriate direction to take in moving towards incorporating biological interactions in management advice (see Recommendation 3). These small, operationally defined systems would serve as the starting point for

iterative analysis and redefinition of the system. In a nutshell, start small and build on it. It was pointed out that taking such subsystems out of context could lead to unpredictable consequences and that there was therefore an element of experimental management in moving this way at any time in the future.

There was a concensus among participants on the final day that large multispecies models such as those being explored by ICES or by National Marine Fisheries Service, Northeast Fisheries Center for Georges Bank were beyond the scope of the resources and manpower of individual Atlantic Region laboratories.

Simple, whole system models were considered briefly from the point of view of using system production constraints to modify single species advice. Examples of these are overall surplus production models and particle size distribution models. They were considered inappropriate at this time for the following reasons: a) our inability to identify systems adequately, b) the outputs are often in terms which do not relate to the current, or forseeable management regimes i.e. catches of individual species, c) there was as yet no evidence that these approaches could provide usable information on total system performance.

Throughout the discussion there was continual reference to the role of MEES. MEES should begin to function more explicitly as a forum for peer review of topics and studies of potential management importance. Such topics come up from time to time in assessment meetings of other subcommittees but often do not get the review they deserve owing to time constraints. An annual meeting of MEES would be one possible forum for advance review of these studies and recommendations concerning their suitability for application in the next round of assessments. Environmental influences on catch and recruitment is one class of topics which falls in this category. More specifically there was discussion that the implications of recent work by Koslow and past work by Sutcliffe and colleagues should be explored in the near future (see Recommendation 1).

Recommendations

The workshop recommended that in general MEES function as a forum for peer review of multi-species and environmental information which could be of potential importance in modifying scientific advice to management. To achieve this MEES should hold an annual meeting with, in each year, a few main focal areas in which progress could be reviewed.

Towards this end the following activities were suggested:

a) That a working group consider the practicality of using some of the available methods of modelling mixed fisheries (technological interactions) to modify single species advice, and report to MEES at the next annual meeting.

b) That a few specific small systems of potential management importance be identified and that progress in these be reviewed annually until the results can be incorporated into the scientific advice. Initially, the cod-capelin system and the silver hake-haddock system were identified requiring closer attention.

c) That the subcommittee review the potentially important area of the influence of environment on recruitment. More specifically: recent work on broad scale environmental patterns and their possible influence on recruitment; and past models by Sutcliffe and how well they have performed in retrospect.

Finally, the Workshop expressed a general concern over the lack of an appropriate forum for interaction of biologists with expertise in relevant socio-economic fields.

In summary, it appears that the first two objectives of the workshop have been met but that no specific management recommendations were possible on the basis of the available information. This was primarily due to the broad scope of the workshop. However, the subcommittee could foresee such recommendations coming forward in the near future from a working group on technological interactions. There was agreement on a strategy for bring systems related advice into the advisory process.

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SESSION I:

HISTORICAL OVERVIEW

Present Management Strategies in Canadian Atlantic Marine Fisheries, Their Rationale and the Historical Context in Which Their Usage Developed

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Management strategies currently in use in Canadian Atlantic marine fisheries are described using groundfish, herring, capelin, and squid to illustrate specific points. The biological basis of various strategies, including mesh size regulations, size limits, TACs and effort regulations, is outlined and the history of their development is described. Mesh size regulations based on the Beverton and Holt yield per recruit model are currently in use for groundfish and total allowable catch (TAC) regulation for groundfish, capelin, and herring based on either Beverton and Holt yield per recruit model or Schaefer model. A number of invertebrate species are regulated by size limit, whereas squid are managed by a combined TAC and effort regulation. These strategies appear to have been successful in rebuilding depleted groundfish resources but have not been successful in preventing severe declines in pelagic resources. Management of squid resources is too short to draw conclusions.

Introduction

This paper is an account of the present status of marine fisheries management in Atlantic Canada in a biological context, and of the process by which we have arrived at this situation. It is intended to provide a basis from which discussion of new, presumably better, approaches can proceed. The paper concentrates on describing management strategies which have been put to practical use, and the context of their implementation, firstly with regard to their biological basis and secondly with regard to pertinent historical and political circumstances.

The definition of management strategies and distinctions between strategies, objectives, tactics, issues, policies, regulatory actions or mechanisms and management tools, are fertile ground for semantic arguments. How matters are viewed depends much on the standpoint of the beholder. We will use the terms objectives, strategies and tactics. Objectives are the goals to be achieved by managing fisheries and can be defined in economic, social and political terms. In the present context of biological aspects of fisheries management, we think that strategies should be defined as relating to control of the distribution of fishing mortality, F. Distribution is used here in a broad sense to encompass control of F over species, stocks, time and size of fish. The word tactics will be used here to define actions taken to achieve the defined distribution of mortality i.e. the management (usually regulatory) tools (mechanisms) employed. These control mechanisms can relate to control of input, i.e. the amount and characteristics of the fishing effort which is available to be deployed, control of behaviour of fishing units at sea e.g. with regard to time and area fished, and control of outputs, i.e. size, quantity and condition of fish landed.

The effective practice of fisheries management requires development of integrated packages of objectives, strategies and tactics. As stated in a recent DFO policy document "effective management is the art of the possible" (DFO, 1981). Biologists have a role to play in all three aspects of management but they are primarily purveyors of strategies. Based on their understanding of the factors affecting fish production, it is they who specify how F should be distributed to produce results consistent with objectives. This is the primary topic of this paper. It cannot be dealt with in an historical context without reference to objectives and availability of management tools (implementable tactics) - the strategies can be fully understood only in the context of the whole package.

New strategies which reflect new knowledge and greater insight into the factors controlling fish production can be developed in isolation, but run the risk of being of little practical utility. Efforts should concentrate on development of strategies which have some prospect of being implementable. Support for research into new strategies is dependent on the expectation of increased benefits, viz. that

present objectives will be more effectively attained or more ambitious objectives can be espoused. Demonstration of the value of the research requires an evaluation of the effectiveness of present strategies in meeting objectives and a prognosis that development of new strategies will be cost effective. These topics are largely outside the scope of the present paper but the effectiveness of present strategies is addressed in the discussion.

Biological Basis of Present Management Strategies

The Single-Species Constant Parameter Approach

Management strategies relate to determination of the distribution of F - the level (amount) of F applied to different species, over different sizes (ages) of animals of the same species, and over time - with the intention of optimizing fishing yields in relation to some particular objective. The phraseology used to articulate management strategies has been heavily dependent on the particular mathematical models used by biologists for determination of resource status and potential yields. Models which require input parameters for only one species, i.e. single-species models, have received much more use than have multi-species models. Thus, from the single-species dynamic pool yield-per-recruit model (Beverton and Holt, 1957) and the general production model (Schaefer, 1954) have been derived the most commonly used reference points in fisheries management, F_{max} and $F_{0.1}$ and F_{msy} and 2/3 F_{msy}, respectively. It is the yield-per-recruit (Y/R) model which has provided the basis of strategies concerning release (or avoidance of capture) of small fish and it is normal practice to refer to strategies concerning the mean size, l_c (or age $-t_c$) at first capture, the Y/R model terminology, when discussing this topic.

Figure 1, showing the yield curves derived based on the models referred to above, is extracted from the recent DFO discussion paper on Atlantic fisheries policy (DFO, 1981). This figure also introduces, in its simplest form, the concept of F_{mey} (fishing mortality giving maximum economic yield), defined as the point on the yield curve where the difference between the value of the catch (proportional to yield or Y/R) and the cost of catching (proportional to fishing effort or fishing mortality) is maximum. F_{msy} is the F giving the maximum sustainable yield and F_{max} is the F giving the maximum yield-per-recruit. If recruitment is not affected by fishing, F_{max} and F_{msy} will coincide.

$F_{0.1}$ is the level of F at which the increase in yield (marginal yield) by adding one more unit of fishing effort is 10% of the increase in yield that would have been attained by adding the same unit of effort when the stock was lightly exploited. The level of F generated by two-thirds the fishing effort required to generate MSY (2/3 F_{msy}) is an arbitrarily chosen F level associated with the general production model (Doubleday, 1976) which is considered about equivalent to $F_{0.1}$ from the Y/R model.

Regulation of mean size at first capture is based on maximization of yield in Y/R calculations. For any given mean size at first capture there is a characteristic Y/R curve (Fig. 2). The model typically defines a maximum Y/R at intermediate l_c and F values.

A more general terminology can be (and sometimes has been) used which encompasses these reference points as special cases. F_{opt} is simply the optimum F, OSY is the optimum sustainable yield and F_{OSY} is the F which gives the OSY. Once "optimum" has been defined, the level of yield and F associated with it can be defined.

Implicit to the above strategies is the idea of managing for a constant level of F. Strategies are also required for dealing with transitions. Transitions have usually involved moving from a situation of high F to a lower value, say $F_{0.1}$, which is considered to be a more suitable value in relation to objectives. The converse has also had to be dealt with, however, in cases of stock recovery. In the Northwest Atlantic, strategy for reduction in F has usually been to adjust F fully in one year, maximizing immediate losses while moving as quickly as possible to the defined optimal situation. The implications of this approach were described by ICNAF's Standing Committee on Research and Statistics (STACRES) in 1975. The figure they used (ICNAF 1976a) is reproduced here as Fig. 3. The length of the transition period is a function of the number of years-classes making a significant contribution to the fishery. In the Northeast Atlantic, in contrast, advice from ICES has frequently been couched in terms of phased reductions, and hence longer-term adjustment periods. Stock recovery situations in the Northwest Atlantic have usually been dealt with by a phased increase in F to the defined optimum in recognition of the high uncertainty which pertains in such situations. For example, the TACs for Div. 4VsW and Div. 4TVn cod were set below levels corresponding to fishing at $F_{0.1}$ during their recovery phase from the low stock sizes of the mid-1970s. In the case of Div. 2J3KL cod, TACs were set which corresponded to estimated F = 0.16, 80% of the $F_{0.1}$ level, to promote faster stock rebuilding.

Figure 3 also illustrates the implications of moving from a high exploitation rate down to $F_{0.1}$ versus F_{max}. The immediate reduction in catch is proportional to the reduction in exploitation rate, but the long-term catch at $F_{0.1}$ is not much less (about 10%) than that at F_{max}. Stock size increases more rapidly at $F_{0.1}$ and to a higher level than when fishing at F_{max}. The transition period does, of course, remain the same.

Long-term strategies based on variable F have also received discussion but have seldom been adopted in practice. Fishing a stock heavily when biomass is high due to good recruitment and fishing at a lower level or ceasing to fish when biomass is low, has been termed "pulse fishing". This was the fishing strategy adopted by distant water fleets of eastern European countries prior to imposition of fixed F management strategies. Pulse fishing is a strategy well suited to maximizing utilization of large mobile factory trawlers with catholic tastes in product while constant F strategies lend stability to small boat fisheries based on a few local resources. There has recently been a suggestion that an F_{max}, or even $F_{0.1}$, strategy contributes to growth overfishing in stocks subject to high recruitment variability (Sinclair et al., 1983). The solution is presumably adoption of a form of pulse fishing strategy. Strategies based on harvesting surplus production or, conversely, maintaining an optimum spawning stock size are also variable F strategies, but these are discussed below under stock-recruitment considerations.

The Single-Species Approach Incorporating Density-Dependence

Single species dynamic pool models which take into account density-dependent responses in recruitment, growth and natural mortality are available based on a variety of hypotheses concerning the form of the density-dependent relationships. Deficiencies in observational data have not allowed discrimination among hypotheses and hence have discouraged widespread practical application of such models in the advisory process. (Although general production models theoretically take into account the complexity of ecological interactions, they have not, in practice, been considered to provide sufficiently precise answers and dynamic pool models have been preferred whenever sufficient data exist for their utilization.)

In contrast to natural mortality, growth in fish stocks is readily measured. There is clear evidence for density-dependent growth in, for example, Northwest Atlantic cod stocks (Beacham, 1982; Lett and Doubleday, 1976; Wells, MS 1984), but this has not been taken into account in the stock assessment models routinely used to generate scientific advice. It has, however, been taken into account in two other ways. Growth parameters used in $F_{0.1}$ calculations are long-term average values which are more likely to approximate those which could prevail at stock sizes associated with fishing at $F_{0.1}$ than are those associated with the much lower stock sizes which have prevailed in more recent history. On the other hand, mean weights at age used for short-term projections are the average of the most recent 2-3 years and thus they should track fairly closely (with some lag) changes resulting from stock density effects. These procedures do not, and other procedures which might take density-dependent growth more directly into account may not, change the management strategy adopted which, for cod, is fishing at $F_{0.1}$. They will allow more accurate definition of the F which corresponds to $F_{0.1}$.

Stock size and recruitment are also fairly easily measured for fish stocks but the relationship between them is complex and confounded by the large effect of environment on levels of recruitment. As a result, relationships based on empirical data have not proved sufficiently convincing to be used as a basis for management advice. When the major finfish stocks in the Northwest Atlantic are fluctuating within their historical range and there is no clear evidence of declines in productivity, it has normally been assumed that there is no relationship between stock size and recruitment within that range of stock sizes (or the question has been ignored, which amounts to the same thing). Nonetheless, while mathematical models incorporating stock-recruit relationships are not in use for these major finfish stocks, declining trends in stock size and recruitment outside historical ranges have evoked serious concerns over potential impairment of stock productivity. A management strategy of minimizing F has been adopted on occasion for several major stocks of cod, haddock, herring and capelin. The decision to move to a conservative strategy has thus been more reactive than anticipatory, and judgments have been based on intuition rather than mathematical calculation. Various guidelines have been proposed for when spawning stock size considerations would over-ride yield considerations in determining management strategy. These have primarily concerned ratios of present (or predicted) spawning stock size to maximum historical spawning stock size calculated from available data series, ratios of 0.05-0.25 having been considered. Such ratios have been considered in the decision making process but a rigid approach based on a particular ratio has been avoided.

There are several cases where management strategies are (or have been) based on maintaining an "optimum" spawning stock size. In the case of Div. 2J3KL cod, part of the stock recovery strategy in the late 1970s-early 1980s

was to increase spawning stock size to within the range 1.2-1.8 million tons with 1.5 million tons as the reference point (ICNAF 1977). This has been achieved by setting TACs corresponding to F's below $F_{0.1}$ and the management strategy has now reverted to one of $F_{0.1}$. The stock size strategy for Div. 2J3KL cod was based on the simple observation that, historically, these stock sizes had corresponded with good recruitment. In contrast, simulation models incorporating complex stock-recruit relationships were developed for Div. 4TVn cod (Lett, 1980 and earlier papers) and these were briefly used as a basis for advice on optimum stock sizes. The models were found to have little predictive capability (Doubleday and Beacham, 1982) and fell into disuse after 1978.

In the case of species which die after spawning, spawning stock size considerations are particularly relevant. Atlantic salmon are managed on the basis of a minimum required spawning escapement strategy. Required spawning escapement is based on calculated egg deposition required for full utilization of freshwater rearing area. This is presumed to maximize recruitment and hence future yields from surplus production. Similar strategies could be applied to, say, capelin but other considerations have taken precedence (see below).

Snow crabs provide a case in some ways similar to salmon, although they do not die after spawning as do salmon. Size at first capture for snow crab has been set sufficiently high (for marketing purposes) that no females are captured and males are only captured after being permitted to spawn at least once, thus ensuring full utilization of egg production potential (Elner and Robichaud MS 1983). Exploitation of the recruited population (males) at 50-60% ($F_{0.1}$) appears to result in close to full fertilization and the overall strategy clearly avoids recruitment overfishing whereas it may or may not maximize recruitment, and hence useable fishery yields.

Squid, both Illex and Loligo, are among the most short-lived of commercial species (less than 2 years) and spawning stock size has been of major consideration in developing management strategies. As stock-recruit relationships are not known, ones were chosen on theoretical grounds which gave moderate dependence of recruitment on stock size (ICNAF 1975e, 1976b). Practical methods (tactics) for control of annual escapement were not available and hence the chosen stock recruitment relationships were incorporated in the population models of Sissenwine and Tibbetts (1977). These indicated that an exploitation rate of about 40% would give maximum sustainable yield, and this exploitation rate has thus been adopted as the management strategy for both species.

The Multispecies Approach

To the present authors, a multispecies approach to fisheries management advice is simply one which utilizes knowledge concerning more than one species. A multispecies approach has been used most commonly in addressing harvesting, or technological, interactions - primarily bycatch problems. Biological interactions, in the form of predator/prey relationships, have also received some consideration. Taken together, technological and biological interactions have been referred to as species interactions, and this has frequently tended to obscure the nature of the matter under discussion. Consideration of biological interactions on some comprehensive basis gives an eco-system approach, whereas a yet broader view, including economic and sociological considerations, gives what could be called a fisheries-system approach.

Harvesting interactions can arise from physical conflicts between gear types fishing for different (or the same) species in the same area and these are referred to as gear conflicts. The more common and troublesome interactions arise from the use of gears such as trawl nets, however, which are poorly selective with regard to species caught.

The management strategy consistently adopted in regard to such problems has been to minimize interactions, a strategy consistent with a single-species management approach. Gear conflicts can only be resolved by spatial/temporal separation of their fishing operations, and tactics for doing this are normally quite effective. Bycatch problems are more ubiquitous and less tractible. In a regulatory context they first arose with imposition of trawl mesh size regulations. The basic tactic adopted has been to allow an incidental catch of mesh-regulated species in non-regulated fisheries, usually 10% of the total catch in a particular area. In the southern half of the NAFO Area (Subareas 4-6) small-mesh trawl fisheries have been of such a large scale that additional measures have been adopted to protect the productivity of large-mesh-regulated species. Small-mesh-gear fisheries are allowed only in restricted season/area boxes. Introduction of TAC regulations introduced a new element to bycatch problems when allocations of various species to particular sectors of the fleet did not correspond to traditional catching patterns. This has been addressed both in the allocation process and again through bycatch regulations. For several years prior to extensions of fisheries jurisdiction ICNAF established a second-tier TAC for Subareas 5-6 fisheries which was less than the sum of individual species TACs - a measure intended, among other things, to encourage a more directed fishing pattern.

The biological basis for advice on solution of harvesting interactions is knowledge of temporal/spatial distributions of the species

involved and the degree of mixing in commercial catches. Observations on the latter are particularly difficult to interpret, however, as commercial catches reflect a combination of fishing strategy and fish distribution.

Biological interactions have received quite extensive debate in the scientific advisory process and this has influenced advice given, but has, as yet, seldom resulted in adoption of management strategies which explicitly take biological interactions into account. Lett (1980 and earlier papers) developed mathematical models postulating various interactions between herring, mackerel and cod in the Southern Gulf of St. Lawrence. Management strategies for herring and mackerel continued to be based on the single-species Y/R model nonetheless. Management strategies for cod in the period 1976-78 were, however, based on a model which incorporated an influence of mackerel biomass on cod recruitment but, as mentioned above, these models proved to have little predictive capability (Doubleday and Beacham 1982) and an $F_{0.1}$ management strategy for Div. 4TVn cod was reverted to in 1979.

Evidence has been brought forward concerning predator-prey interactions which appear of sufficient importance that they should perhaps influence management strategies of cod versus American plaice and snow crab in the Gulf of St. Lawrence (see e.g. Bailey MS 1981; Waiwood and Majkowski 1984), and shrimp versus cod and Greenland halibut off Labrador (see e.g. Parsons MS 1983). They have not as yet done so. The trophic inter-relationships associated with capelin have received the most extensive study and it is one case where the importance of a species as prey for other, commercially more important, species has been explicitly recognized in the management strategy adopted. A low exploitation rate strategy was adopted for capelin from the initiation of directed fishing for it and, when analytical stock assessments became possible, this translated into an exploitation rate of 10%. It was specifically stated in 1981 by the NAFO Scientific Council that this exploitation rate "recognizes that capelin are an important source of food for predators, especially cod." (NAFO 1981). The 10% exploitation rate is not based on a quantitative multi-species model incorporating trophic dynamics; it is entirely arbitrary but is nonetheless explicit recognition of biological interactions in an implemented management strategy.

Management strategies for harp and hooded seals have been based on yield and stock size considerations whereas management of harbour seals (through a bounty system) was concerned with reducing interference with inshore fisheries based on a strategy of reducing population size. Management considerations for grey seals in recent years have centred around biological interactions between grey seals, seal worm and cod. The strategy proposed for management of grey seals has been to reduce population size perhaps to that of the late 1960s, a level of half or less of current levels (CAFSAC 1982), but no action has yet been taken. The biological basis for the proposed strategy again is not a quantitative model of interactions.

The major expansion of international fisheries in the 1960s-early 1970s resulted in development of management strategies based on ecosystem considerations (ICNAF 1973d, 1975d). The resultant tactics were imposition of second-tier TAC regulation in Subareas 5-6 and fishing effort regulation in Subareas 2-4. The underlying biological basis was heavily dependent on multispecies general production models (Brown et al. 1976; Halliday and Doubleday 1976; Pinhorn 1976) but the Subareas 5-6 considerations included a multispecies Y/R model and the relationship of primary to finfish production (Au MS 1973). Extensions of fishery jurisdiction created conditions under which control of international fishing effort at about the level giving MSY from the ecosystem as a whole was no longer the focus of management attention, and these earlier attempts were not persued. More complex system models have not yet generated the development of management strategies which have received application, and hence they are not the basis for "present" management strategies.

History of Present Management Strategies

In tracing the history of management strategies presently in use, we will restrict the discussion to groundfish, pelagic (herring and capelin), and squid resources, which illustrate highlights in the development of strategies to manage marine resources in the Northwest Atlantic. Discussion will include the rationale for the management strategies adopted at the time as well as the historical context in which their usage developed.

A. Groundfish

The groundfish fishery, one of the oldest fisheries in the Northwest Atlantic, has been prosecuted by European fishermen from the time of the very early settlement of eastern Canada. In fact, Newfoundland was settled in the beginning because of the desire of European fishermen to catch the abundant cod in Newfoundland waters. Fishing in the early days of settlement and until the middle of this century was almost solely with baited hooks on the offshore banks from schooner-type vessels and with bar seines, cod traps, and cotton gillnets in inshore areas from small inshore boats. With the early 1940's came the advent of the otter trawler and by the late 1940's it became obvious that fishing pressure on the groundfish stocks in the area was rapidly building. The United States of America

convened a conference of 11 countries in Washington in January 1949 to consider problems affecting the fisheries of the Northwest Atlantic. The work of this conference resulted in the opening for signature on 8 February, 1949 of the International Convention for the Northwest Atlantic Fisheries. With the signing of this Convention the International Commission for the Northwest Atlantic Fisheries, ICNAF, came into being. The area for which ICNAF had responsibility is shown in Fig. 4. Designated areas (e.g. 2J), known as Divisions, were established on the basis of stock boundaries and served to subdivide the area for purposes of collection of fisheries statistics. The 200 mile limit is also shown on this map.

As pointed out above, for a given level of fishing mortality, there exists an age (size) at first capture which maximizes yield per recruit for a given species and which depends on the natural mortality and growth rate of that species. In the early days of fishing in the Northwest Atlantic, levels of effort were moderate and even in the the early 1950's ICNAF was concerned primarily with reducing wastage of small fish at sea. In 1953 ICNAF introduced the first regulations restricting the size of mesh used in the codend of the otter trawl, so that mesh sizes below a certain specified size could not be used (minimum mesh size regulations). In its first 15 years, ICNAF established some 20 regulations, all concerned with mesh size (and trawl construction) limitations. The biological basis for these regulations was provided by ICNAF's Standing Committee on Research and Statistics (STACRES) and is largely summarized in Beverton and Hodder (1962). Fishing pressure increased very rapidly in the late 1950's and throughout the 1960's however (Fig. 5), resulting in substantial increases in F so that, even in the few cases where these minimum mesh sizes were being rigorously enforced, mesh size was below the optimum for the prevailing level of F. Therefore, the beneficial effect of mesh size regulation was not fully realized, even though the minimum mesh size was increased throughout the 1960's and early 1970's.

ICNAF realized at an early stage that in addition to minimum mesh size regulations, some control would have to be placed on the amount of fish being removed. STACRES warned the Commission in 1964 that further regulatory measures were required to check the rapid expansion of fishing effort in the ICNAF Area (ICNAF, 1964a). As a result the Commission asked for a review of "the various kinds of action which might be taken by the Commission for the purpose of maintaining the stocks of fish in the Convention Area at a level at which they can provide maximum sustained yields "(ICNAF, 1964b). The conclusion from the review by Templeman and Gulland (1965) was that there must "be some direct control of the amount of fishing. All methods of doing this raise difficulties, but that presenting least difficulties is by means of catch quotas. There must be separate quotas for each stock of fish, e.g. for cod at West Greenland, and preferably be allocated separately to each section of the industry". ICNAF basically agreed but launched into detailed study through its Working Group on Joint Biological and Economic Assessment of Conservation Actions (in conjunction with FAO, NEAFC, and OECD) (ICNAF, 1968), and its own Standing Committee on Regulatory Measures (STACREM). The first catch limits (called total allowable catches, TACs) were agreed to in 1969 for application to haddock on the southern Scotian Shelf and Georges Bank in 1970.

These were overall catch limits or "global" TACs in that countries were not allocated shares. All could fish until the TAC was taken. This presented obvious enforcement difficulties, potential economic inequities, and the promise of fisheries disruption, all of which were well recognized, but ICNAF was constrained by its convention which allowed only overall catch limits (and, incidentally, espoused the objective of achieving the "maximum sustained catch"). The Commission, also in 1969, initiated revision of its convention which allowed "---appropriate proposals--- designed to achieve the optimum utilization of the stocks---". This amendment also included economic and technical considerations, as well as scientific, as a basis for regulatory proposals. In addition to changing the objective of the Commission from MSY to OY, this amendment allowed national allocation of catch quotas. It came into effect on 15 December 1971 and was utilized in February, 1972 with agreement on catch quotas and national allocations for herring in Subareas 4 and 5 (ICNAF 1972a). This "broke the dam" of resistence to TAC regulation and catch controls were in effect in 1973 for the major groundfish stocks in the Canadian Atlantic area (ICNAF 1972b) and by 1974 all groundfish stocks had come under TAC regulations (ICNAF 1973a). Therefore, ICNAF had achieved international agreement on the level of TACs to be set on all major stocks within its jurisdiction, as well as the sharing of these TACs among member countries. This represented a major breakthrough in international fisheries negotiations.

These first TACs were based on advice provided by STACRES (ICNAF 1972c, 1973b). The management strategy implied by this

advice up to 1977 was fishing at F_{max} or F_{msy}, depending on whether the stock was assessed using the yield-per-recruit or general production models respectively. In other words, the target fishing mortality was to be set to extract every ton of fish possible from the stock. (Although the Commission's objective became OY in 1971, this continued to be translated as meaning MSY).

Although an international inspection scheme was in place within the ICNAF regime, whereby inspectors from one country could board vessels of another country and inspect their catch, the inspection effort was so low that a true picture of the total fish caught was not possible by this method. Instead, it was essentially left to each individual country to enforce its own allocation and report its catches to ICNAF. There is sufficient evidence to conclude that national enforcement efforts were frequently less than adequate.

Judged on the basis that the decline in catch rates resulting from a decline in the abundance of stocks, which began in the mid 1960's, was not arrested by the TACs imposed by ICNAF in 1973-76 (Fig. 6), the overall management system put in place was not effective. Since the catches reported were less than completely accurate, the accuracy of assessments of the state of stocks on which the TACs were based was adversely affected, but other problems such as time lags in obtaining data and insufficient data also created stock assessment problems. Whether assessment or enforcement deficiencies were to blame for the lack of response by the ecological system was not clear. However, in recogniton of these apparent shortcomings in the F_{max} (or F_{msy}) strategy, STACRES at its 1976 Annual Meeting (ICNAF 1976a) based its advice for TACs for 1977 on fishing at $F_{0.1}$, not F_{max}. Several reasons for managing stocks at a level of fishing mortality less than F_{max} or F_{msy} were pointed out. These were (ICNAF 1976b):

i) Errors associated with TACs can be large, and losses from over-exploiting a stock are likely to be much greater than any losses due to under-exploitation.

ii) Fishing at higher levels of fishing mortality reduces the number of age groups in the stock with the result that the fisheries (and the calculated TACs) are heavily dependent on recruiting age-groups. This increases the probability of error in the TACs.

iii) Although it was too early to fully assess the effects of regulations in recent years based on F_{max}, it was evident in many cases that the stocks were continuing to decline.

Implementation of TACs at the $F_{0.1}$ level resulted in significant reductions in TACs for most stocks (eg. for Div. 2J3KL cod the TAC at F_{max} in 1976 was 300,000 t while at $F_{0.1}$ in 1977 it was 160,000 t, a reduction of almost 50%). As May et al. (1980) pointed out, the extent to which acceptance of these reduced TACs represented agreement among the member nations that this was a more appropriate management strategy is hard to discern. Agreement may have been heavily influenced by the fact that reduced TACs were strongly advocated by an influential coastal state, Canada, on the eve of her extension of jurisdiction. (The announcement by Canada that fisheries jurisdiction would be extended to 200 miles was made just before the 1976 ICNAF Annual Meeting).

The move to fishing at $F_{0.1}$ was sponsored by Canada. Canadian support was based on economic reasons, eg. increased catch rates, larger fish, lower processing costs, as well as the conservation reasons given by scientists. Canada also took another major initiative during the last years of ICNAF - institution of fishing effort control for groundfish in Subareas 2-4. The Canadian proposal was introduced at the 1975 Annual Meeting of the Commission (ICNAF 1975a) and called for a reduction in fishing effort on groundfish stocks in Subareas 2, 3, and 4 in 1976 of 40% from the 1973 level. The proposal received intense debate at the Seventh and Eighth Special Meetings in September 1975 (ICNAF 1975b) and January 1976 (ICNAF 1976d) and was implemented for 1976 by agreement at the meeting of that January. The proposal received many modifications before implementation and, in any case, excluded reductions in effort for coastal states. Thus, the expected overall impact in terms of reduction in fishing effort was substantially less than 40% from the 1973 level. (The actual impact has not been evaluated but Fig. 5 suggests that a reduction by perhaps as much as one-third occurred between 1973 and 1976 in Subareas 2-4). Nonetheless, Canada held the view in the mid 1970's that not only was single-species F_{max} management inadequate to "optimise" Northwest Atlantic fisheries, but that a much lower exploitation rate was required for each major species and that control of total exploitation rate, at least

of the groundfish component of the ecosystem, was also required.

In the meantime, the Third United Nations Conference on the Law of the Sea had been discussing aspects of international law relating to the oceans of the world, among these being coastal states' rights to renewable resources off its shores. The discussions within the Law of the Sea Conference had reached such a stage by 1976 that Canada was in a position to declare a 200 mile Fisheries Zone, effective 1 January, 1977. Canada's position at the Law of the Sea(LOS) Conference was in fact an extension of coastal state jurisdiction to the edge of the continental shelf but this was not supported by the conference as a whole. Declaration of a 200 mile Fisheries Zone meant that Canada, as a coastal state, had the right to determine the appropriate harvest levels for fisheries occurring within 200 miles of its coastline and had first option on yields from stocks of fish within its zone. The draft LOS text required that fish surplus to Canadian needs be allocated to other countries. This declaration of a 200 mile Fisheries Zone allowed Canada not only to set TACs (and other regulations) within its zone and to allocate shares of these TACs to various countries, but to institute controls in the form of licences to foreign vessels, with numbers of days allowed within the zone, and beginning and ending dates, specified for individual vessels. This ensured that catches taken within the zone would be within the limits of allocations given.

The strategies adopted by Canada on extension of jurisdiction did not differ from those developed in the last days of ICNAF but the effectiveness of controls (regulatory effectiveness) increased greatly. That this resulted in more effective management of groundfish stocks within the 200 mile zone is suggested by the fact that catch rates, which reached a minimum in 1975 in the Canadian Atlantic area, have returned to levels prevalent in the 1960's (Fig. 6). Also, although total tonnage of vessels decreased by 40% in the period from 1977 to 1980 (Fig. 5), catches increased by 12% in the same period (Fig. 6).

The 200 mile limit did not include all of the continental shelf off Canada's coast, but, in fact, excluded all of Flemish Cap (Div. 3M) and portions of the Grand Banks, known as the "nose" of the Bank in Div. 3L and the "tail" of the Bank in Div. 3N (Fig. 4). A new international commission, known as the Northwest Atlantic Fisheries Organization (NAFO), was formed in 1979, to deal with fisheries in these areas outside 200 miles, which are still subject to international regulation as in the days prior to 1977. Canada has one vote among 13 countries concerning TAC levels and national allocations for those stocks completely outside its zone in the Flemish Cap area. For stocks which extend outside the 200 mile limit in the area of the Grand Bank, Canada has control only over the portions of these stocks which are inside 200 miles. Therefore, it has attempted as a member of NAFO to maintain uniformity in regulations inside and outside the zone and has participated internationally through NAFO in the setting of TACs for overlapping stocks.

Effectiveness of Canadian control within its 200 mile limit since 1977, compared to the international control practiced outside 200 miles, is suggested by the rapid increase in the abundance of cod stocks inside 200 miles compared to the lack of increase on the Flemish Cap and the very slow rebuilding on the southern part of the Grand Banks where cod overlap the 200 mile limit.

B. Herring

Herring stocks in Div. 5Z+SA6 (Georges Bank), Div. 5Y (Gulf of Maine) and Div. 4WX (Nova Scotia) were the first to be subjected to TACs with national allocation in 1972. This major breakthrough in international fisheries negotiations was the result of a recognition that the herring stocks in the southern Canadian and northern U.S.A. areas were depleted. Although in retrospect actual TACs were set too high and in fact were set higher than the scientific advice at the time (ICNAF 1972c) , the fact that TACs were set at all in the international climate prevailing at the time is remarkable. It was at this time also that the concept of $F_{0.1}$ (or F_{opt} as it was first known) was introduced, initially to deal with the situation in herring where yield per recruit curves are typically flat-topped and thus F_{max} is ill-defined (i.e. for technical rather than management reasons) (ICNAF 1972d).

At the Special ICNAF Meeting in 1973 (ICNAF 1973c), a target biomass level was also established for Georges Bank and Gulf of Maine stocks and this may have been the first international regulation of its kind. The text from that meeting read as follows for Georges Bank herring (page 28):

> "That the Commission establish the level of catch for 1974 which will result in the restoration of the adult stock to at

least 225,000 MT by the end of 1974, it being understood that in any event the level of catch for 1974 will not be increased above that for 1973 unless the adult stock size at the end of 1973 has reached a level which will provide the maximum sustainable yield by the end of 1974".

That these management measures were not successful in achieving the desired objective is evidenced by the decline in TACs (reflecting declining stock size) for Georges Bank herring from 150,000 t in 1975 to 60,000 t in 1976 and eventually to zero after 1977 and for Gulf of Maine herring from 25,000 t in 1974 to 16,000 t in 1975, 7,000 t in 1976 and scientific advice for zero TAC in 1977 (ICNAF 1976b). Although unsuccessful, this represented the first attempt at maintaining a minimum target biomass while fishing biomass surplus to this. Of course, subsequent to 1977, Gulf of Maine herring have been managed by the United States and Scotian Shelf herring by Canada, under their 200 mile jurisdictions, whereas management of Georges Bank herring has been in dispute. Since extension of jurisdiction Scotian Shelf and Gulf of Maine stocks have been fished by Canada and U.S.A. respectively but no directed fishery for herring has been permitted on Georges Bank because of a total stock collapse and no sign of recovery.

C. Capelin

Although capelin had been harvested for centuries by coastal fishermen for bait and garden fertilizer, it was not until 1971 that a commercial fishery developed on the southeast shoal of the Grand Bank (Div. 3N) when USSR vessels caught 750 t (Winters and Carscadden 1978). Total catches increased from 3,000 t in 1971 to 71,000 t in 1972 and to nearly 270,000 t in 1973 (ICNAF 1976b). Because of this rapid increase in catch, ICNAF instituted a TAC for capelin in 1974 at a level of about 250,000 t (an actual TAC was not set but country allocations summed to 258,000 t with the proviso that any country without a specific allocation would be limited to 10,000 t) (ICNAF 1974a). Although the scanty scientific evidence at the time indicated a much larger catch could be harvested (as much as 750,000 t) (ICNAF 1973b), the TAC was set at this lower level to allow for a slow development of the fishery but also recognizing that capelin represent perhaps the major forage species in the Northwest Atlantic, being preyed on by a wide variety of fish species, mammals and seabirds (Winters and Carscadden 1978). The following year (1975) the scientific advice was for 500,000 t (300,000 t in SA2+Div. 3K and 200,000 t in 3LNOPs) (ICNAF 1974b) but the ICNAF Commission, although agreeing to 200,000 t in the southern area, set the TAC in the northern area (SA2+Div. 3K) by allocating 160,000 t to USSR and a maximum of 10,000 t to each country without a specific allocation (ICNAF, 1975c). This in fact resulted in a catch in SA2+Div. 3K in 1975 of 199,000 t. The scientific advice for the following year (1976) was to set the TAC at 300,000 t in SA2+Div. 3K and 200,000 t in Div. 3LNOPs and maintain it at this level for three years until sufficient scientific data could be collected to properly assess the status of the stock (ICNAF 1975d) . This scientific advice was repeated for 1977 and 1978 (ICNAF 1976b and 1977). This the Commission did and the TACs remained at 300,000 t in SA2+Div. 3K and 200,000 t in Div. 3LNOPs for the years 1976-78. Unfortunately, by the time sufficient scientific data had been collected to adequately assess the stocks in 1979, the abundance of capelin had declined from natural fluctuations in recruitment so that the scientific advice for 1979 was for a 75,000 t TAC in SA2+Div. 3K, 16,000 t in Div. 3L and no directed fishing in Div. 3NO (ICNAF 1979). The TACs for 1979 were actually set at 75,000 t for SA2+3K (CAFSAC 1984b) and 10,000 t for Div. 3LNO (NAFO 1979). The TAC in Div. 3LNO was actually for Div. 3L only since no directed fishery was permitted in Div. 3NO. The TAC advised for Div. 3L recognized that capelin populations will exhibit large fluctuations in biomass and since they are important in the diet of many species, during periods of poor recruitment, the exploitation rate should be low to protect the spawning stock (ICNAF 1979). Thus, the advice was based on a conservative exploitation rate of 10% of the estimated biomass in Div. 3L. This management strategy for capelin has prevailed to the present and the 1985 TACs were in fact advised on the basis of the 10% exploitation rate (directed fishing for capelin in Div. 3NO has not resumed since its closure in 1979).

Thus, management strategies for capelin, as noted in the previous section, not only recognized the importance of multispecies interactions in managing fisheries, which has been known for many decades, but for the first time (at least in the Northwest Atlantic) represented direct action to account for such interactions in managing fisheries.

D. Squid (Illex) (Subarea 3+4)

Like capelin, squid had been harvested by coastal fishermen for bait purposes for centuries, especially in the Newfoundland area. However, it was not until 1975 that a commercial fishery for squid commenced when catches increased to 18,000 t. Regular increases occurred to 1979 when catches reached 102,000 t (Subareas 3+4). Catches then declined to 70,000 t in 1980, 32,000 t in 1981, 13,000 t in 1982, and to only 400 t in 1983. Such fluctuations in catches are typical of the squid fishery and reflect large fluctuations in abundance caused by variations in recruitment. Since squid are believed to live only one year (ICNAF 1978), fluctuations in recruitment are reflected proportionately in fluctuations in abundance in the same year. This peculiarity of squid biology makes it impossible to predict abundance any earlier than several months before the fishery. Thus, it is not possible, as it is for groundfish, to estimate a TAC associated with a particular fishing mortality in any given year for the following year. Recognizing this, ICNAF imposed pre-emptive TACs of 25,000 t in 1975-77 (ICNAF 1974c, 1975a, 1976c). However, in 1978 and 1979 TACs were calculated based on the abundance of squid in the previous year and a 40% exploitation rate, which was deemed to be the appropriate level of exploitation from yield per recruit studies (see earlier section) (ICNAF 1978, 1979). In discussion at the 1980 meeting, NAFO realized the futility of such an approach when squid abundance varies so greatly from year to year. Therefore, in 1980, NAFO devised management tactics, whereby a TAC of 150,000 t would be set regardless of the abundance of squid in a given year. This TAC was derived by relating TAC levels of 100,000 t, 150,000 t, and 200,000 t to estimates of biomass for the years 1968-1979. It appeared that a TAC of 150,000 t would not be associated with a serious risk of excessive exploitation (ie. greater than 40%) in most years (NAFO, 1980). Imposition of such a TAC in 1980 was combined with effort regulation whereby the numbers of days a country was licensed to fish was calculated on its share of the TAC in 1980 and its catch rate in 1979 when squid abundance was known to have been high. This management strategy thus had a built-in safeguard in that if the abundance of squid in a given year was very much lower than in the previous year, the offshore fishery would not be able to maintain high catch rates and the limitation of fishing effort based on the previous year's catch rate would ensure that fishing mortality would not greatly increase despite reduced abundance. The price paid to ensure no overexploitation by the offshore fleet in years of low abundance is substantial underexploitation in years of high abundance. Since the inshore fishery is basically self-regulating, high effort being expended when squid are abundant and vice-versa, it was felt that excessive exploitation would not occur in the inshore fishery.

Thus, the management tactics adopted for squid attempted to deal with managing a species, which shows wide fluctuation in abundance, without having the capability of predicting such fluctuations in advance of the fishing year. It is apparent that the squid resource is now in a state of reduced abundance, as has happened several times in the recorded history of this fishery. Whether the management strategy of adopting a target exploitation rate and the tactics for preventing this target from being greatly exceeded, have been successful in protecting the productivity of the stock will be judged by whether squid abundance continues to follow the same cycles of abundance as in the past and thus increases in abundance again in the not too distant future.

Discussion

There is a perception in some sectors of the scientific community that present management strategies have an inadequate scientific basis and that the fisheries management objectives being strived for cannot be attained through their use. On the other hand, fisheries managers on the whole appear to perceive relatively few deficiencies with the present scientific basis for management and, in particular, are not convinced that a multi-species approach to management will provide benefits commensurate with research costs involved. To achieve the widespread application of the present scientific basis for management, it was a prerequisite to build client confidence in the product. This, and the apparent successes in groundfish management in terms of stock recoveries in recent years, have combined to produce a fairly high level of confidence in fisheries managers for present products. (This is not to be confused with expressions of dissatisfaction by the fishing industry at Departmental efforts as a whole.) This may be looked upon as a mixed blessing by those looking for additional funding to investigate new multi-species or ecosystem approaches. Recent history has shown, however, that management systems in the Northwest Atlantic have been highly adaptable to evolving scientific philosophies of fisheries management. Presumably this will continue and, once the practical benefits of new approaches have been demonstrated, additional support will be attracted.

The perception among some scientists that present management strategies have an inadequate scientific basis needs more examination to put the situation in perspective. Limitations of presently utilised population models are universally recognised, but in a practical management context, the question is not how good (realistic) they are, but whether they are good enough. Adequacy is to be judged in the context of the problem being addressed. In making a judgement about management strategies it is also necessary to try to distinguish success or failure of strategies themselves from success or failure in their application i.e. in tactics. Thus, while success of management as a whole may be judged on the end results in terms of catches, catch rates, stability, etc., or on more general conditions of economic viability of industry sectors and levels of employment produced by them, ascribing causes requires detailed investigation.

Present day Canadian Atlantic fisheries management approaches developed very largely in the context of Northwest Atlantic international fisheries in the 25 year period from 1950. In this large uncontrolled experiment in fisheries management, it is difficult, and no doubt in some cases impossible, to separate out particular cause and effect relationships. The quality of information used to describe events is variable and even some large scale events could have been obscured due to deficiencies in statistical reporting. Detailed scientific observations on the dynamics of the major stocks seldom extend back prior to 1960 - a brief period in relation to the time scales over which stocks can be expected to show major fluctuations or cycles in productivity and these natural cycles could confound interpretation of the effects of management actions. Thus, although the record of this period from 1950 deserves to be thoroughly sifted for insights into the impacts of management actions, the analysis will inevitably tend to be speculative and the results inconclusive. Despite these many cautions, it is instructive to look at overall results of management to see what major questions come to light.

The groundfish story is summarised in Fig. 5 and 6, which show a substantial decrease in vessel tonnage and fishing effort from about 1974, minimum Canadian trawler catch rates in 1975 with subsequent increase to the highest values on record by 1981, and an accompanying increase in total catches from a minimum in 1977. The predominant groundfish species is cod and trends for groundfish in total follow quite closely trends for Canadian cod stocks, which have previously been documented by May et al. (1980). Nonetheless, there is evidence, at least for the southern half of the NAFO area (Subareas 4-6), that the groundfish community has tended to return to a species composition, distribution, and abundance not greatly different from that before the heavy fishing pressure of the 1960s and early 1970s (Brown and Halliday 1983). These results correspond fairly closely with the kind of results aimed for when management measures were adopted. It is possible to argue that there was a natural increase in resource productivity coincident with implementation of management measures and that the improvements in resource status are fortuitous. Environmental trends show coherence over large parts of the Northwest Atlantic (see e.g. Trites 1982) and it is possible, for example, to erect hypotheses concerning enviromental influences on fish production which are supported by high statistical correlations between environmental signals and production (e.g. recruitment) indices (see e.g. Sutcliffe et al. 1977). To whatever degree it may prove to be the case that environmental factors or fishery regulatory actions have been responsible for historical resource trends, it is unlikely that the benefits from the recent increase in groundfish abundance would have been realised in terms of improved catch rates, rather than in a temporary increase in catches, without management intervention.

Pelagic fish stocks give rather a different story. The major species which occur from Georges Bank north, (herring, mackerel, and capelin) all supported large international fisheries for brief periods. (The only other large fishery for a pelagic species in the Northwest Atlantic is that for menhaden, and the fishery and its management has been entirely a USA concern. It occurs primarily in the Middle Atlantic Bight.) Initial concentration on herring resulted in peak catches in 1969. Emphasis then switched to mackerel and then to capelin with peak catches in 1973 and 1975 respectively. In total, catches of the three species were stable between 1968 and 1975 at 1.0-1.2 million tons (Fig. 7). Provisional catches for 1983 of these three species are 232,000 t. It does not seem likely, from available evidence, that the fishery was substantial enough to greatly influence the dynamics of capelin stocks and the simultaneous collapse of the major stocks has been ascribed to natural recruitment failure. Most recent evidence suggests that a resurgence may be underway (e.g. NAFO, 1983) but this is not yet reflected in catches. In contrast, mackerel stocks were heavily exploited in the early 1970s, stock size was greatly reduced and, although recent Fs have been low, stock size has remained at a relatively low level (CAFSAC 1984a). The long-term history of mackerel fisheries in the Northwest Atlantic suggests, however, that periods of very high abundance such as that which occurred in the early 1970s are fairly widely spaced temporally (Anderson and Paciorkowski 1980). Present stock size is roughly the same as that in the early 1960s and it is too soon to prognosticate a fishery induced stock failure. Herring stocks have, in most cases, a history of heavy exploitation and substantial stock decline, during the late 1960s and the 1970s. In the extreme, Georges Bank no longer supports

a herring stock and fishing pressures appear to be a primary cause of its demise. Coastal Newfoundland stocks, at the northern end of the species range, receive only occasional good recruitment. Recruitment failure in these northern stocks throughout the 1970s has been the fundamental cause of stock declines, low to moderate fishing pressure only accelerating the process. Coastal Maine and Bay of Fundy stocks are the only ones which are maintaining productivity close to long-term expectations. In total, the overall abundance of herring in the Northwest Atlantic in the early 1980s is at its lowest in the period which can be documented. This is not consistent with management objectives of the last decade.

It would appear from the above that there have been major trends in total pelagic fish biomass in the Northwest Atlantic and we have put together a composite table of biomass estimates (Table 1) as a basis for investigating these trends. Stock size estimates for herring and mackerel in the Northwest Atlantic are comprehensive and reliable at least as far as general trends are concerned. Table 1 excludes small herring resources in Div. 4S and on part of the Scotian Shelf only. Capelin abundance is less well measured. A variety of assumptions concerning acoustic abundance indices, intercalibrations among indices, interpolations and extrapolations were required of the present authors to produce the numbers in Table 1 for the major capelin stocks. Smaller capelin stocks in the Gulf of St. Lawrence are not included. Table 1 also excludes the major pelagic resources represented by menhaden and arctic cod (Boreogadus saida) which overlap the southern and northern parts respectively of the range of the 3 species which are considered in the table. Nonetheless, Table 1 gives a broad overall impression of trends in pelagic fish biomass in Subareas 2-5 and these are also shown in Fig. 8. (Although mackerel overwinter partly in Subarea 6, most of their feeding and reproduction occurs further north.) These data support the thesis that pelagic biomass reached a minimum in 1979. Recent levels are likely higher, entirely as a result of increased capelin abundance, but still much below those in the mid-1970's. If one assumes that capelin abundance in Div. 3LNO was similar in the early 1970's to that in the mid-1970's, then pelagic abundance in the early 1970's is not likely to have been any higher than in 1975-76. Also in this circumstance, biomass in the northern part of the area (Subareas 2-3), which was almost entirely capelin, would have reached a peak in 1975 and a minimum in 1979. In the southern part (Subareas 4-5), there has been continuous decline from 1970 to 1982, the most recent data point. One could also speculate that the pelagic biomass was perhaps not greatly different between the northern and southern areas around 1970, but has, on average, been higher in the north during the 1970's.

To round out the picture, it is interesting to note that there was an expansion of offshore invertebrate fisheries (shrimp, squids, and snow crab) in the 1970s which was apparently supported by an increase in abundance of these resources. This increase was coincident with the decreases in groundfish and pelagics. Management strategy has been to control exploitation rate of each stock at an "optimal" level. It would be difficult to evaluate the success or otherwise of these measures at this early stage. It is tempting to speculate that trophic relationships between groundfish and invertebrates (particularly snow crab and shrimp) alluded to above (op. cit.) may prove of greater significance to management than optimising yields from fishable stocks.

The most general questions which come to mind based on this kind of review, and which are not addressed in any way by present management strategies, are the ones just noted concerning the future of invertebrate fisheries given current strategies for groundfish management, and the future of pelagic fisheries. In particular, the response of herring stocks has been quite different from that of cod stocks despite application of quite similar management strategies for over 10 years. Circumstantial evidence suggests that the large fish - low exploitation rate strategy for groundfish has resulted in benefits much along the lines predicted. This claim requires detailed examination to establish its validity and see what lessons can be learned.

In summary, we have described the biological basis for present management strategies and the historical context in which these strategies were developed and applied. We have gone on to describe the major fisheries trends during the period of their application but cautioned against hasty or superficial judgements of the utility of present management strategies. We do so because it is, of course, important to identify clearly the nature of problems before devising solutions to them. In this case, it is not sufficient to simply illustrate the theoretical limitations of the population models underlying present strategies as a basis for denial of their utility. Neither is failure, or success, of overall management efforts necessarily, of itself, a basis for judgement of management strategies.

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Discussion Period

Silvert: My understanding of what you are saying is that managers believe that multispecies interactions are important and consider them qualitatively in management, but that there has been no way of developing adequate, quantitative predictive multispecies models. Consequently, managers have been unable to include this information about interactions explicitly in their plans. Therefore, our main question here should not be whether we need multispecies models, but how we should set about developing them. One clue to what we should be doing is in your statement that models have been proposed, but they required too many parameters. So I am inclined to focus on some way of constructing models that are not much more data intensive than existing single species models.

Pinhorn: I agree with that. It seems we have two tasks, one is to develop the type of model you have just described, the other is to sell these types of models to the managers. My experience with a previous attempt was that the management system could not cope with complex models.

Mahon: In regard to another point which was made, I would observe that our success in the past with the models we now use has been largely in the rebuilding of depleted stocks. I suspect that as we attempt to manage nearer equilibrium levels, we are not going to be successful. In fact some recent retrospective analyses of our performance tend to suggest that we were quite far off the mark even during the rebuilding phase.

Pinhorn: Following on from that it is obvious that we are trying to do the impossible, we are trying to manage all the stocks out there at an $F_{0.1}$ level. Various interactions may make this impossible and if we maintain the strategy for the next 10-15 years we will probably be in for some surprises.

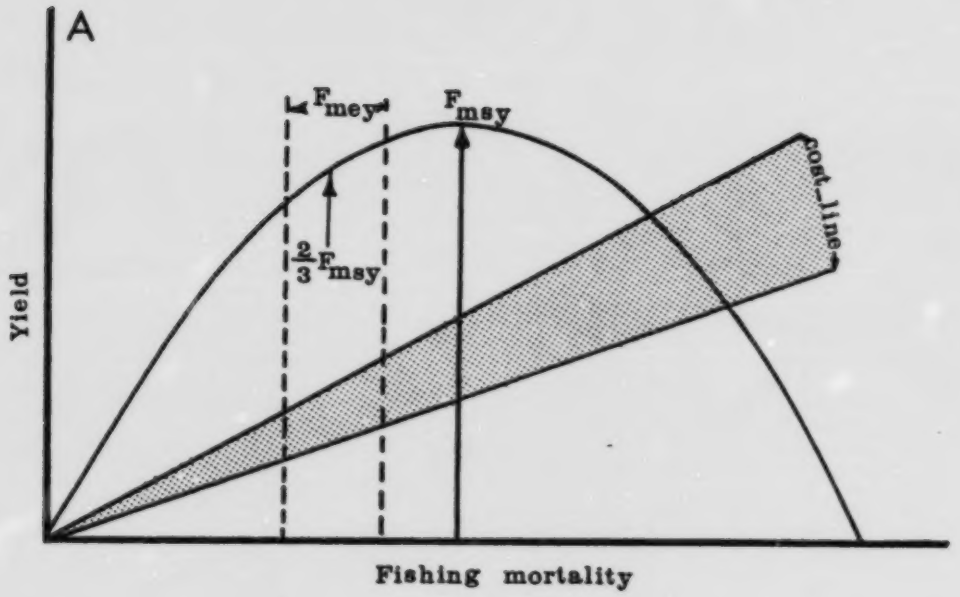
Silvert: You referred several times to predictive power. Could you clarify what you mean by that?

Pinhorn: I was referring specifically to the Southern Gulf cod model which was used in management and failed to predict, over several years, what was happening to the stock.

Dickie: It seems to me in listening to Al Pinhorn's talk that he has made a strong case for one aspect of the multispecies problem. When we classify all the stocks as either groundfish or pelagic and we do see a different response to management, we are accepting, and perhaps the managers are too, that there is a classification where the animals in one group have more in common with each other than they do with those of the other group. It seems that there should be some kind of general model that would reflect this inner consistency and external difference.

Table 1. Estimates of stock biomass (t x 10 $^{-3}$) for capelin (approx. age 3^{+}), mackerel (age 1^{+}) and herring (age 2^{+}) in the Northwest Atlantic, 1970-83. Capelin estimates are for September, others for January.

Species	Area	Year														Source
		1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	
Capelin	2J3K	1055	2160	1987	2251	1848	4025	2843	1474	434	428	1108	1853	1541	2891	Derived from Carscadden et al. 1984 for 2J3K
	3L	-	-	1400	1200	1400	800	2000	1800	1300	600	550	1100	600	600	and from data provided by Carscadden
	3NO	-	-	-	-	-	1050	685	1000	0	0	72	144	446	190	(pers. comm.) for 3L and 3NO. Intercalibrations
	Total	-	-	-	-	-	5875	5527	4274	1734	1028	1730	3097	2587	3681	between data series and interpolations for missing data are by the present authors.
Mackerel	3-6	1755	1606	1566	1075	849	731	525	475	517	525	463	407	422	366	Maguire (pers. comm.)
Herring	E. Nfld.	384	449	470	453	404	338	275	212	157	118	79	58	41	41	Wheeler et al. (MS 1984)
	S. Nfld.	94	98	87	64	50	38	30	27	23	18	13	9	10	10	Moores et al. (MS 1981). Biomass 1981-83 projected.
	4R	295	344	335	286	244	217	196	166	152	136	114	94	70	-	Tremblay et al. (MS 1983)
	4T	728	425	297	246	213	159	164	138	123	91	82	104	85	85	Derived from Cleary (MS 1982, MS 1983)
	4WX+5	837	611	610	669	807	752	509	461	455	528	606	616	603	-	Fogarty and Clark (MS 1983)
	Total	2338	1927	1799	1718	1718	1504	1174	1004	910	891	894	881	809	-	
All Species																
	Total	-	-	-	-	-	8110	7226	5753	3161	2444	3087	4385	3818	-	
	2-3	-	-	-	-	-	6251	5832	4513	1914	1164	1822	3164	2638	3732	Assumes no mackerel in 2-3.
	4-6	3615	2986	2808	2276	2113	1859	1394	1240	1247	1280	1265	1221	1180	-	



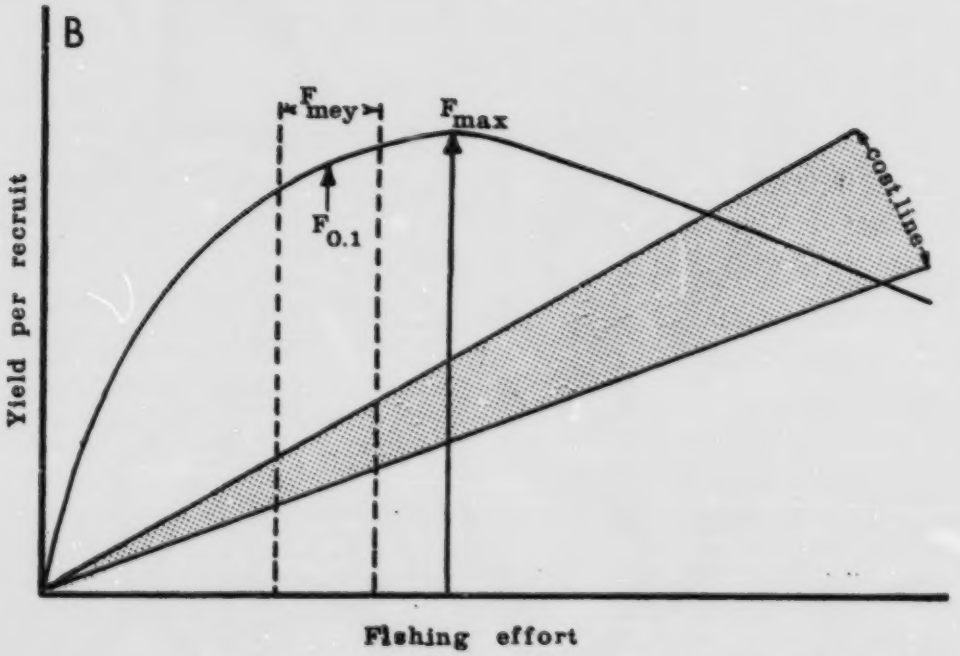


Fig. 1. The two most commonly used yield curves illustrating some strategy options. The cost lines are given as a range to emphasize the point that the precise location of "maximum economic yield" on the yield curve varies with costs. F_{mey} points outside the range illustrated are also possible.

A. General production type curve with F_{msy} and 2/3 F_{msy} illustrated.

B. Yield per recruit type curve with F_{max} and $F_{0.1}$ illustrated. (Modified from DFO, 1981).

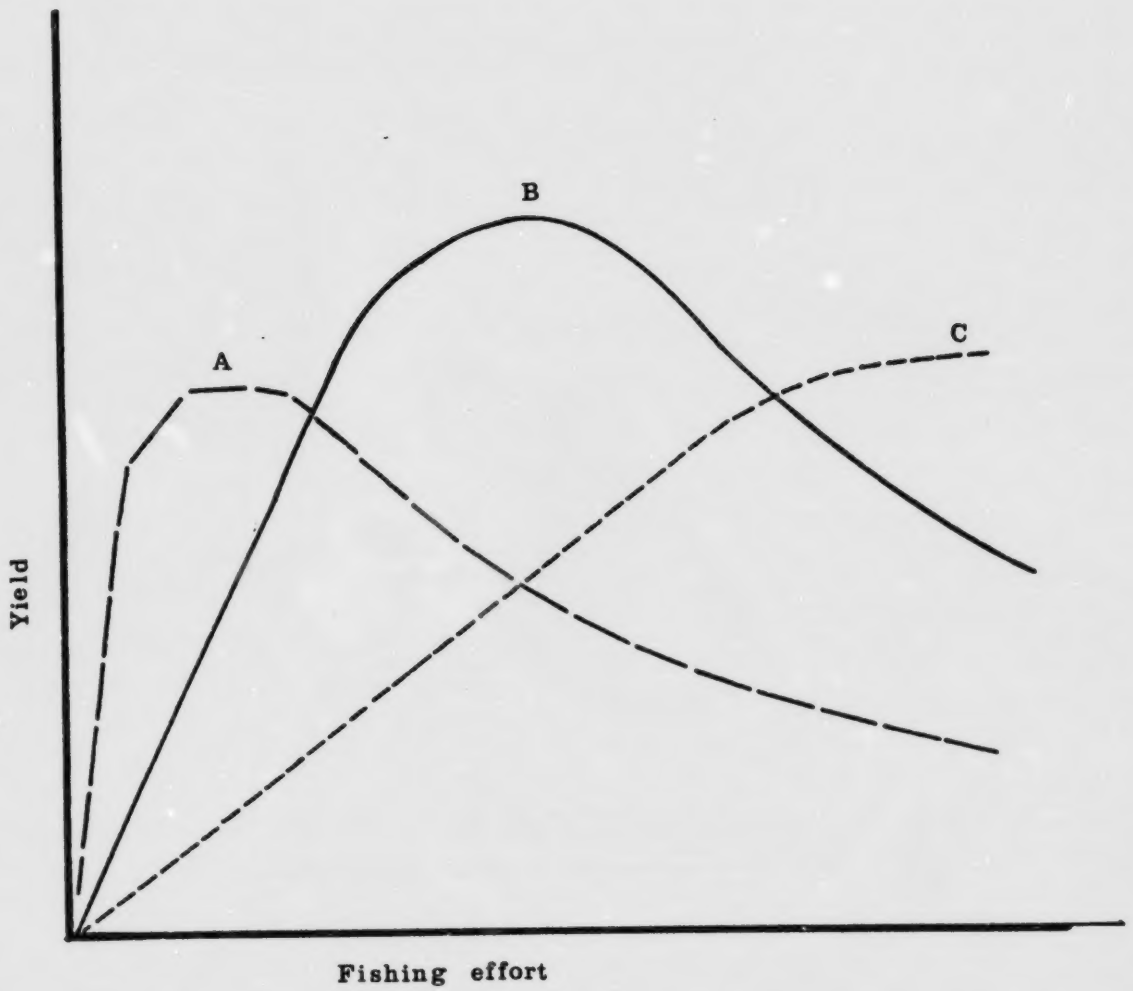


Fig. 2. Yield as a function of fishing effort when: A) catching small fish, B) catching medium size fish, C) catching large fish.

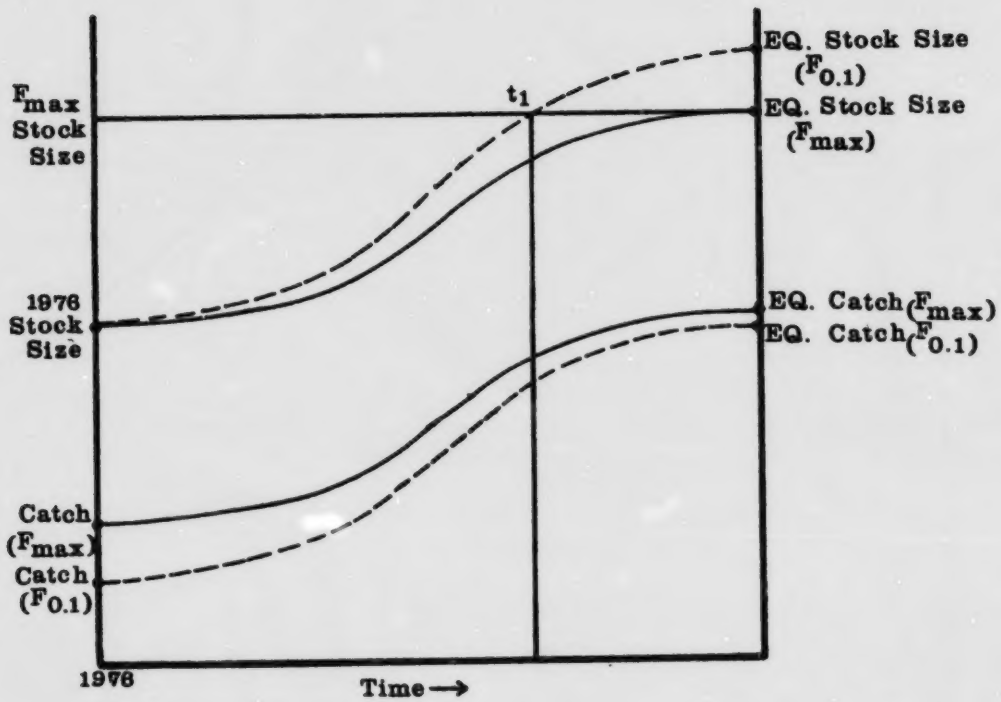


Fig. 3. Changes in stock size and catch with time given a policy of maintaining constant F. Solid lines refer to fishing at F_{max} and dash lines to fishing at $F_{0.1}$, EQ. means equilibrium (from ICNAF 1976a).

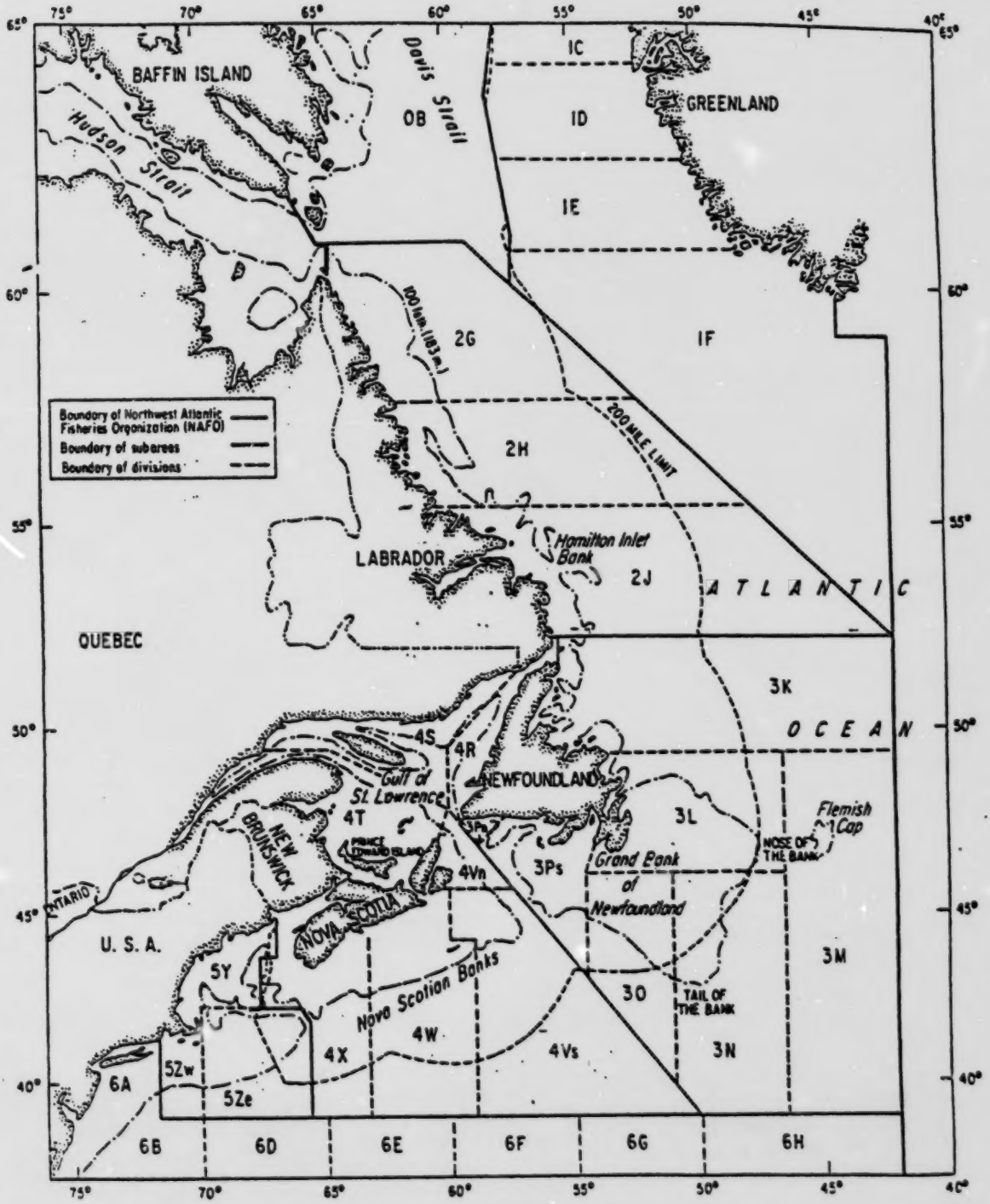


Fig. 4. Map of Northwest Atlantic showing NAFO Divisions and Canadian 200 mile limit.

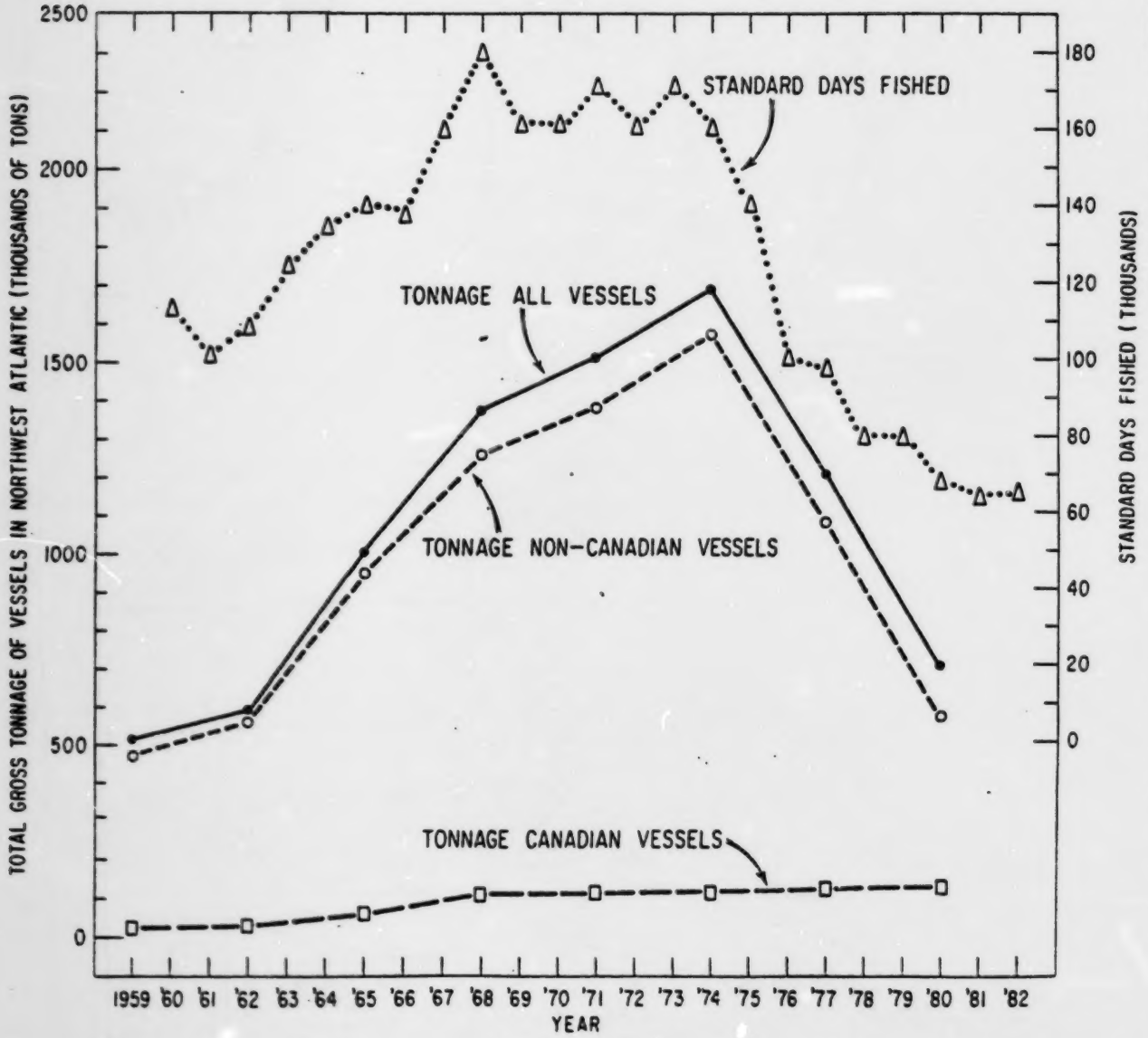


Fig. 5. Total tonnage of vessels $>$50 gross tons fishing in the Northwest Atlantic and standard days fished by these vessels, 1959-82. Coastal state tonnage is underestimated as many vessels were $<$50 gross tons, standard days fished is for Subareas 2-4 only and is calculated from Fig. 6 by dividing total catch by catch per day fished of Canadian otter trawlers, 151-500 GRT.

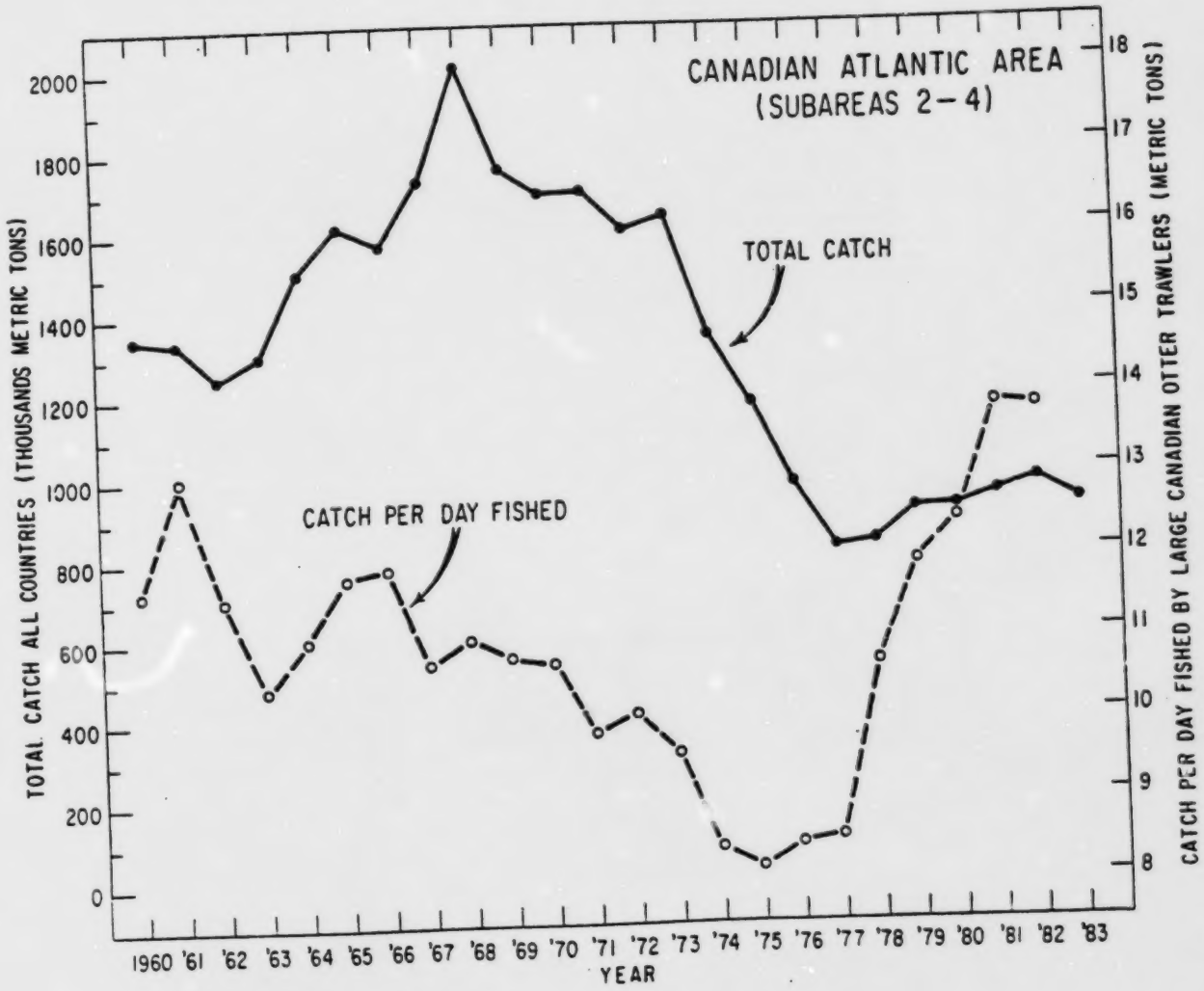


Fig. 6. Total Groundfish catch by all countries and groundfish catch per day fished by Canada otter trawler, 151-1000 GRT, in Canadian Atlantic area, 1960-83. Catch per day fished is calculated from total catch and total days fished by Canadian vessels of 151-1000 GRT irrespective of tonnage class, region of origin, or Subarea fished but corrected for errors in Canada (MQ) 1977-78 and Canada (M) 1979-80 fishing effort reported in ICNAF/NAFO Stat. Bulls. Vol. 27-30.

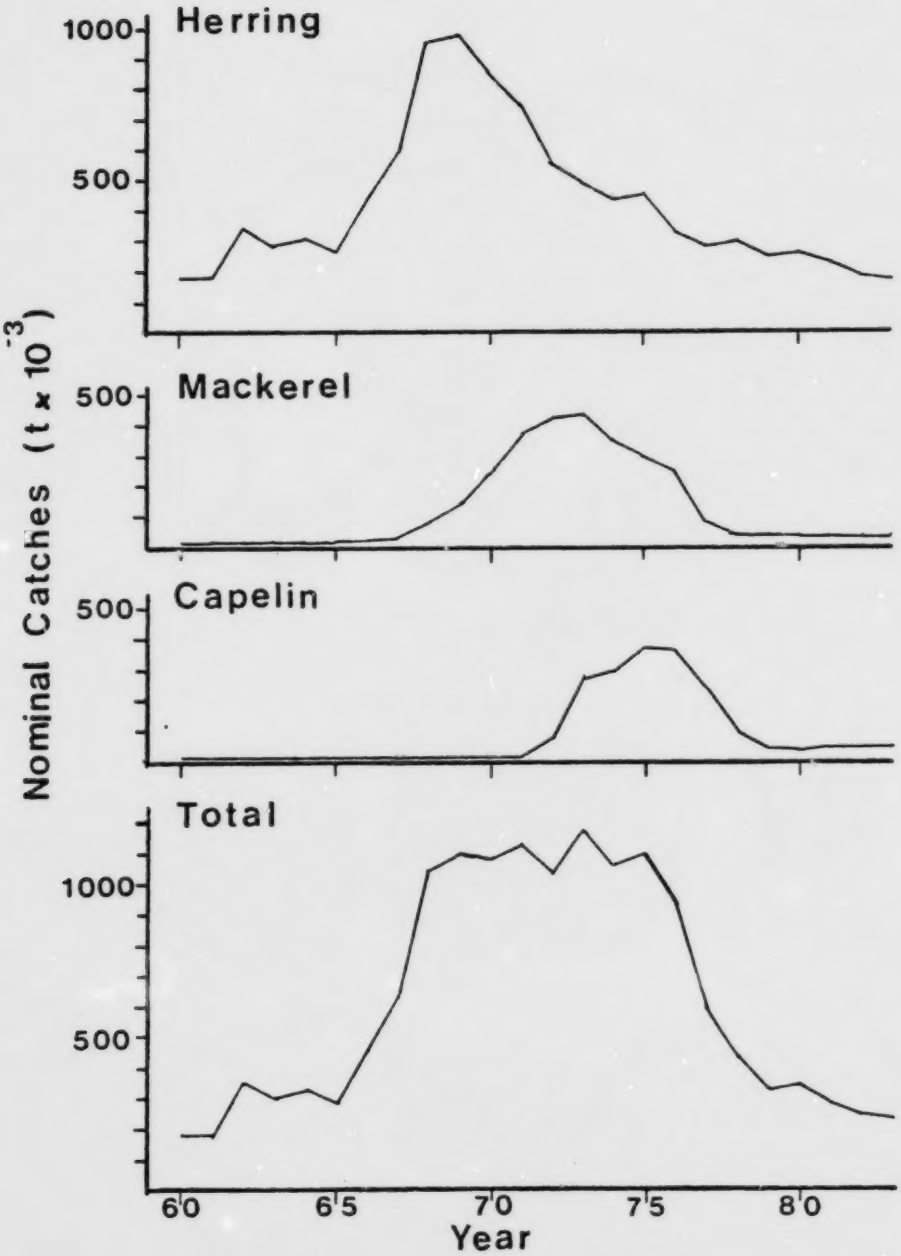


Fig. 7. Nominal catches of herring, mackerel, and capelin in the NAFO Convention Area, and their total, 1960-83.

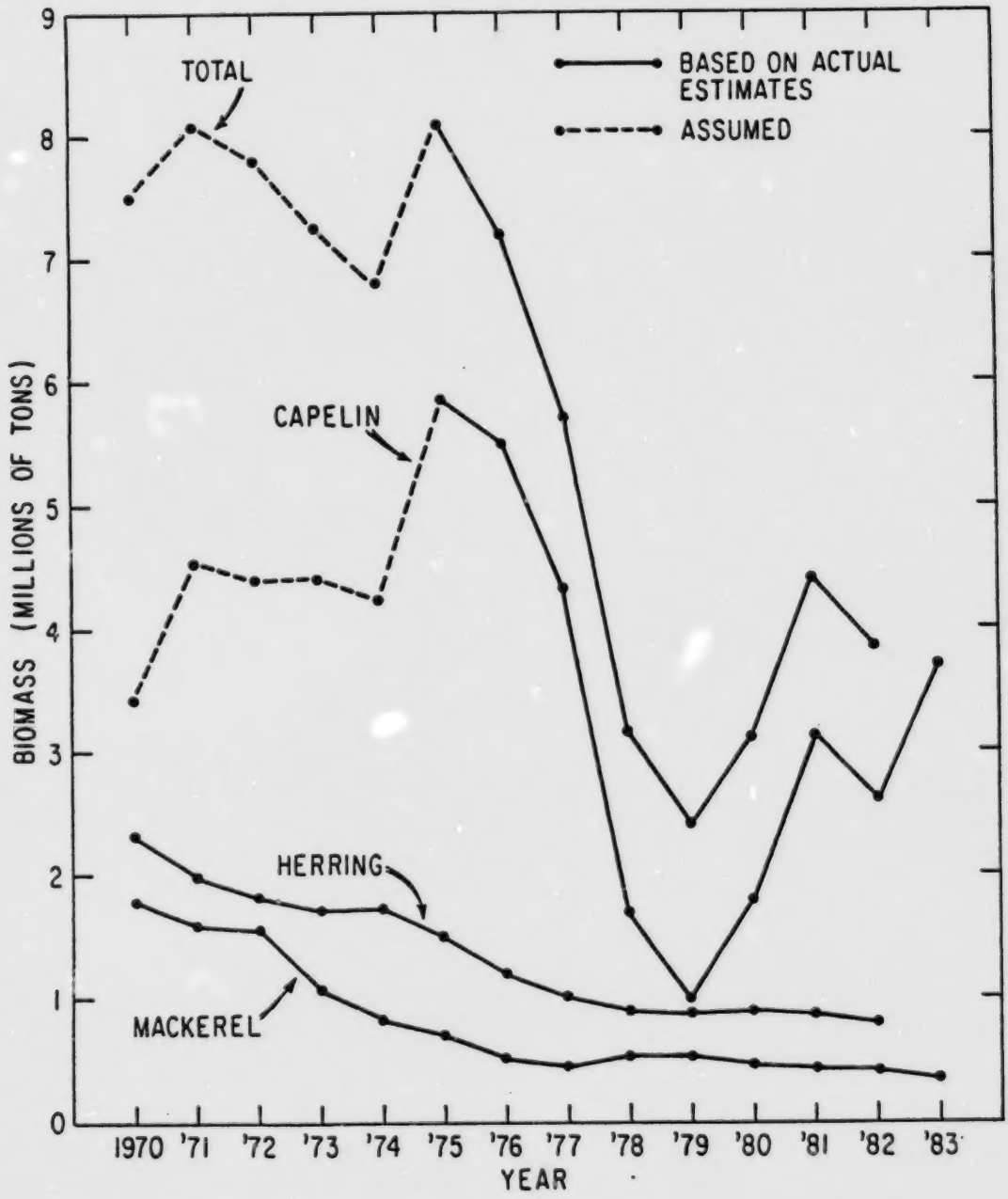


Fig. 8. Trends in stock biomass for capelin (approx. age 3+), mackerel (age 1+) and herring (age 2+) in the Northwest Atlantic, 1970-83. See Table 1 for sources of data.

Review of Reported Biological and Technological Interactions for Commercial Marine Species of the Newfoundland Region

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Anderson, J.T., and G.R. Lilly. 1985. Review of Reported Biological and Technological Interactions for Commercial Marine Species of the Newfoundland Region, p. 34-53. In: R. Mahon [ed.] Towards the Inclusion of Fishery Interactions in Management Advice. Can. Tech. Rep. Fish. Aquat. Sci. No. 1347.

A review of reported biological and technological interactions occurring in marine fisheries of the Newfoundland Region indicated interactions fell into one of two categories: predation effects and by-catches. The influence of reduced capelin abundance on predators has been the area of greatest concern. Although there are indications that variability in capelin abundance affects growth in cod, natality in puffins, and feeding migrations of cod and whales, the results remain inconclusive. Other studies have examined the effects of predator-prey interactions on recruitment and have produced initial estimates of consumption but no clear statements regarding predation as having significant effects on the fishery have emerged. Some studies of species by-catch have indicated cause for concern while most have not. Increases in species by-catch may be brought about by changing abundances, distributions or environmental effects, but most often it is due to changing fishing strategies. These strategies can involve changes in gear technology as well as areas fished. By-catch concerns arise solely due to the single species approach to fisheries management and, as such, these are usually bi-species problems.

Introduction

Multispecies management of commercial marine species has received continued attention as the most promising management option for the future (May et al. 1979; Mercer 1982). It has proven somewhat easier to conceptualize the problem than it has been to suggest pragmatic implementations of management strategies. Some options have been proposed based on species interactions in the North Sea (Andersen and Ursin 1977; Ursin 1982) and mixed fishery considerations on Georges Bank (Murawski 1984). In Atlantic Canada there have been suggestions for multispecies management of cod and redfish in NAFO Subarea 2 and Div. 3K (Pope 1975), and cod and mackerel in Div. 4T (Lett 1978). A recent multispecies workshop in St. John's, Newfoundland addressed the subject in a broad context and suggested guidelines for research program planning towards provision of multispecies management advice (Mercer 1982). However, while the groundwork has been laid, specific recommendations for managing the Atlantic fisheries in a multispecies context are lacking. As a basis of discussion for this workshop it was felt that a review of reported interactions 'experienced' in our area would be useful - the past should point the way to the future. Incumbent on our reasoning is that management of single species cannot be undertaken without considerations (management) of the integral production system (Dickie and Kerr 1982; Tyler et al. 1982).

In a multispecies context, interactions can be classified as one of two types - biological or technological (Anderson 1975; Murawski 1984.) We will define an interaction as an event that has a positive or negative effect on the population biomass of the species in question. Biological interactions can be subdivided into several categories (Odum 1971). Here we consider categories of predation, competition and parasitism as being important. Technological interactions are mostly associated with gear by-catches in a directed fishery of one or more species of commercial value.

Our approach was to survey all CAFSAC Research Documents (1977-84), various ICNAF/NAFO publications (Research Documents 1970-84, Selected Papers, Council Studies), the Journal of Northwest Atlantic Fisheries Science and a series of key words in the Canadian Journal of Fisheries and Aquatic Sciences (1965-84). Due to the unpublished nature of many of the studies reported as ICNAF/NAFO or CAFSAC research documents, study results and conclusions reported here may be open to some interpretation. As such, some of these conclusions are tentative and may be revised with time. We have not included 'known' interactions not specifically reported (e.g. haddock-yellowtail flounder on the Grand Bank), food and feeding studies or methodological papers. For purposes of this review we have concentrated on the Newfoundland Region due both to our familiarity with this region and the fact more interactions have been reported for this region than for either the Scotian Shelf-Bay of Fundy or Gulf of St. Lawrence areas. While our examples are drawn from this region (Subareas 2 and 3) we feel that generalizations and conclusions would largely apply to the entire Atlantic region.

All of the reported interactions in the Newfoundland fisheries were classified as to type and summarized in Tables 1 and 2. Additionally,

each study has been summarized in an annotated bibliography (Appendix 1) while an overall summary and evaluation is given below. Interactions basically fell into one of two categories - predation or by-catch. Thus, the history of reported interactions indicates the types of problems that can be expected in managing multispecies fisheries can be confined to these two categories.

Predator-Prey Interactions

Predator-prey interactions involving commercially important marine species are common and usually easily identified. An interaction may become a concern to management if the fishery for one of the participants alters the behaviour or dynamics of another and the affected species has commercial importance, commercial potential (eg. sand lance), or aesthetic value (eg. seabirds, whales). It is convenient to classify these interactions into those in which we are concerned about changes in prey abundance affecting the predator and those in which we are concerned about changes in predator abundance affecting the prey (Table 1). Of course, in many instances both predator and prey are fished and we may be concerned about an optimal exploitation of the two species.

Influence of prey on predator

Changes in abundance of a species may cause changes in a predator's energy intake, which may in turn result in changes in mortality, growth rate, reproductive success, and so on. Changes in prey abundance may also cause changes in the predator's foraging behaviour.

The multispecies interactions of major concern in Newfoundland waters at present involve capelin and its many predators. Concern arose in the 1970's with the initiation of a large offshore fishery in 1972-73 and the subsequent rapid decline in capelin biomass in the late 1970's following a succession of poor year-classes (Carscadden 1984). Despite the large change in capelin abundance, no relationship was found between capelin biomass and either growth rate of cod (Akenhead et al. 1982) or smolt-class success in salmon (Carscadden and Reddin 1982). The authors of both studies noted that the data available were inadequate or inappropriate for testing the hypotheses. Brown and Nettleship (1984) described reduced breeding success of puffins when capelin were scarce or absent during the chick-rearing stage, but Carscadden (1983) challenged some of their analyses and conclusions. Capelin are also a major prey for harp seals (Sergeant 1973) and baleen whales (Winters and Carscadden 1978), but the relationships between capelin abundance and population dynamics of these species have not been investigated.

Many capelin predators, such as cod (Lilly 1984a; Lilly and Rice 1983) and harp seals (Bowen 1981), feed on a wide variety of other prey, and it has been suggested that such predators might not be tightly linked to capelin. A reduction in capelin abundance might not result in greatly increased mortality or decreased natality in these species, but might be manifested in more subtle effects such as reduced growth rate, increased age-at-maturity, and decreased fecundity, all of which indicate reduced production. Other predators, such as puffins and murres, might have fewer alternate prey (Brown and Nettleship 1984) and in these species the effects of reduced capelin abundance may be more acute. The relationship between capelin abundance and total food consumption has not been adequately studied for any predator. Lilly (1984a and b) found that the rate of predation on capelin by cod in Div. 2J+3K in autumn did vary with capelin abundance. Increased predation on alternate prey only partially compensated for the reduction in predation on capelin.

There has also been concern that a reduction in capelin abundance might affect the inshore cod fishery because the proportion of the cod stock following capelin toward shore would be reduced. There is weak evidence that this has been the case (Akenhead et al. 1982), although more detailed studies are required. There is also an hypothesis that an increase in whale collisions with inshore fishing gear in the late 1970's was due to a change in whale foraging patterns. It is thought that decreased abundance of capelin offshore resulted in a higher proportion of whales coming inshore where spawning concentrations of capelin could still be found, but the data have not been examined thoroughly (Carscadden 1984).

Other examples of the influence of the prey on the predator are probably not of concern to management at present. Lilly (1980) hypothesized that growth rate of cod on the Flemish Cap is related to the abundance of juvenile redfish, and Mercer (1975) reported that peaks in landings of squid and pilot whales were coincidental, which is evidence that the whales follow the squid.

Influence of predator on prey

For convenience, the instances of predation on commercial species have been divided into two categories: (1) those in which the predator preys only on pre-recruits and therefore only affects the strength of year-classes as they enter the fishery, and (2) those in which the predator preys on pre-recruits but also competes directly with the fishery for individuals which have recruited to commercial gear.

The influence of predators on pre-recruits has received increased attention in recent years. Feeding studies in the North Sea have shown that mortality rates of young fish (ages 0-2) are very

high (Daan 1983), and investigators at Woods Hole (eg. Cohen et al. 1984) have presented evidence from Georges Bank that post-larval mortality regulates year-class strength in most years. The only statistical examinations for such effects in Newfoundland waters involve short-finned squid, which are highly variable in their abundance at Newfoundland. Correlation analysis was used to test the hypothesis that squid abundance is inversely related to year-class strength in Atlantic cod, capelin and herring (Dawe et al. 1981; Dawe et al. 1983). The only significant relationship found was for Div. 2+3K capelin, assuming squid prey on capelin age-groups 0+1 (Dawe et al. 1981), and even this relationship may be non-significant if more recent data are added (Carscadden 1984). There is also evidence that cod consume large numbers of small redfish on the Flemish Cap (Lilly and Gavaris 1982; Lilly 1983), but the effect of this predation on year-class strength will be difficult to determine without estimates of absolute abundance during the early juvenile stages of redfish. Other instances of predation on pre-recruits include predation on snow crab (Lilly and Rice 1983; Lilly 1984b), an interaction which has been studied more thoroughly in the southern Gulf of St. Lawrence (Waiwood and Majkowski 1984). Cod and Greenland halibut prey on juvenile groundfish, including cod, Greenland halibut and American plaice (Lilly 1983, 1984a; Bowering et al. 1984), but no attempts have been made to determine the effect of this predation (including cannibalism) on year-class strength.

Instances in which predators and the fishery take individuals of the same size have received considerable attention. Again, the species attracting most attention has been capelin. Winters and Carscadden (1978) produced a surplus production model which was intended to provide a first estimate of potential long term annual yield of capelin. They estimated the annual consumption of capelin by predators in the 1950's and 1960's and again after predator populations were reduced in the 1970's, and calculated by difference the quantity of capelin which was in excess of predator requirements in the 1970's and would therefore be available for a fishery. Other estimates of consumptionof capelin have been made for cod (Minet and Perdou 1978; Turuk 1978; Lilly et al. 1981), seals (Sergeant 1973) and marine birds (Brown and Nettleship 1984). The estimates are based in all instances on inadequate sampling of predator stomachs, inadequate information on predator ration and, in most instances, inadequate estimates of the size of predator populations. The consumption by other predators, such as Greenland halibut (Lear 1970) and American plaice (Pitt 1973), has not been estimated. There has been no attempt to compare capelin production in a specific year with capelin consumption by all predators, and there has been no examination of annual variability in capelin consumption by a specific predator.

Other planktivorous fish which have commercial potential are sand lance and Arctic cod. Sand lance is a major prey of cod on the Grand Banks (Popova 1962; Lilly and Rice 1983), and Winters (1983) has found a negative correlation between cod biomass and sand lance biomass. Arctic cod are prey for both cod and Greenland halibut off Labrador and northeast Newfoundland (Bowering et al. 1984; Lilly 1984a), but little is known about changes in its abundance.

Two invertebrates, shrimp and squid, are preyed upon before and after they enter the fishery. Bowering et al. (1984) made gross estimates of the consumption of shrimp by Greenland halibut and cod on shrimp grounds off Labrador, but the estimates need to be refined and should be compared with shrimp production, which is not known. Cod prey on squid (Lilly and Osborne 1984), but again any estimate of consumption by the cod population would be very gross with present information. Mercer (1975) provided some order-of-magnitude estimates of annual squid consumption by pilot whales.

General considerations

In aquatic systems the roles of predator and prey are largely a matter of relative size, and in several instances there are reversals of role during ontogeny. For example, squid consume small cod (Dawe et al. 1983) and large cod eat squid (Lilly and Osborne 1984). Both cod and Greenland halibut prey upon juveniles of the other species (Bowering et al. 1984; unpublished data). Cod, Greenland halibut and squid are cannibalistic (Lilly 1983; Bowering et al. 1984; Dawe et al. 1983). Because the role of each species in the food web may change during ontogeny, any examinaton of predator-prey interactions must consider the size structure of the interacting populations.

Although the predator-prey interactions reviewed in this paper involve a restricted part of a food web (eg. capelin and its predators; shrimp and its predators), any management deliberations should include implications for species somewhat removed in the food web. For instance, the present policy with respect to harvesting capelin is to be conservative (10% exploitation rate), partly in recognition of the importance of capelin to predators (Anon 1982). Thus, we limit the catch of capelin to ensure that a high biomass of cod can be sustained, yet a high cod biomass may reduce the yield from other commercial species such as shrimp and crabs.

The annotated bibliography (Appendix 1) includes brief statements of any problems or limitations noted by the authors, and Carscadden (1983) discusses many limitations specific to studies involving capelin. Some of the problems are as follows:

1) A comment frequently made by investigators who wished to test specific hypotheses regarding multispecies interactions is that the information available from present sampling programs is often insufficient or inappropriate. Carscadden (1983) has suggested that such interactions be the subject of directed, well-planned, independent studies.

2) A major problem is obtaining accurate measures of population sizes of both predator and prey, and determining the spatial and temporal scales of overlap between the species.

3) Stomach content information is inadequate. Even for the cod-capelin interaction, which has been studied more thoroughly than any other, there is no estimate, based on adequate seasonal and spatial sampling, of the contribution of capelin to the total food consumption of a single cod stock in a given year. There is very little information on annual variability.

4) Information on metabolic rate and gastric evacuation rate, required for calculation of consumption rate, remains inadequate or non-existent for most predators.

5) Mortality rates of juveniles cannot be calculated because there are no measures of abundance during early juvenile stages. This is a problem even in the NAFO Flemish Cap study, which is directed at examining factors affecting recruitment.

6) Foraging behaviour of all predators is poorly understood, so it is difficult to predict how the predators will react to changing prey availability.

Competitive Interactions

There is no indication in studies reviewed here that species competition, whether through direct interference or resource use, has been a concern in fisheries management. In fact, competition may not be a concern in the context of multispecies management. In natural situations species will evolve towards positive interactions (Odum 1971) and hence a host of mutualistic, symbiotic, commensal relationships. While competition ultimately cannot be ignored, particularly in heavily fished areas where the 'balance' may be upset, there is no clear evidence that it exists in marine systems (Dickie and Kerr 1982). In the case where stocks collapse, such as California sardine, re-colonization has not taken place possibly due to competitive interactions. Such man-induced competition that is not desirable prompts the obvious management response - do not fish stocks to commercial extinction.

Parasitic Interactions

Studies of parasites have largely been descriptive and related to considerations of stock identification (e.g. Pippy 1969; Bourgeois and Ni 1983). One study examined the effect of harbour seal bounty kills on cod worm infestation (Wiles 1969). While the conclusion was that there was no noticeable reduction in infestation it was pointed out that the bounty kill data came from areas not known for cod worm infestations and that harp and grey seals had been ignored in terms of their effect on infestation. It appears parasitic interactions are not important although this may not be the case elsewhere (cf Mansfield 1981; Brodie and Beck 1983).

Gear Interactions

Management concerns raised in by-catch studies in the Newfoundland Region have largely involved removals of a species of commercial importance during a directed fishery for another species. The question most often posed is: do these by-catches represent a significant source of fishing mortality? It is probably fair to say most directed fisheries produce a by-catch of one or more species. These range from non-significant catches (Bowering 1981a, 1982) to fisheries that are best described as mixed fisheries (Brodie 1981, 1983) and may range to such things as the swordfishery off Nova Scotia which can be more aptly described as ". . . a shark fishery with a by-catch of swordfish" (Brodie and Beck 1983). The studies reported here have been mostly a management response to fishing concerns that a significant by-catch of a particular commercial species is not being accounted for (i.e. managed). This often occurs with changing fishing strategies although it is not specific to gear type, having been reported for trawls, trap nets and gill nets, but may also occur with changing environmental conditions and species abundances.

Of the nine studies considered here four reported there were no major by-catches or concerns while four reported there were and one indicated not at the present time (Table 2). Only in one case were the data considered wanting to the analysis (Stevenson et al. 1984), but conclusions regarding the importance of the by-catches were reached nevertheless. In no case was a statistical test of significance reported. This alone points to one of the major shortcomings of testing for significant interactions from available data, it is not stochastic. The reason most often given for a non-interaction was geographic separation of the two species in question with respect to the area(s) in which the fishery was carried out. For example, yellowtail flounder were not considered to be a major by-catch in the directed fishery for American plaice as this has largely been carried out in Div. 3L in recent years where yellowtail are less abundantly distributed

(Brodie 1983). In contrast, Greenland halibut has increased as a by-catch in the roundnose grenadier fishery apparently due to decreasing water temperatures (400-1000 m depth) and increasing abundance of Greenland halibut moving into areas previously occupied by the grenadier (Chumakov and Savvatimsky 1983, 1984). This points to the importance of geographical distributions, migrations and in particular how these may change. Both seasonal and yearly patterns will be important and it therefore becomes obvious more information on these distributions will be necessary in managing multispecies fisheries. Another observation is that by-catch concerns reported invariably have dealt with two species only, and never has consideration been given to species of non-commercial importance. This begs the question of how "multi" our approach to multispecies management must be. Are we to expect most interactions will occur between two species or is this an artifact of available data and past observations? The fact that changes in a by-catch situation may occur is demonstrated by both the yellowtail-plaice and halibut-grenadier by-catches mentioned previously. One was due to a changing fishing strategy, the second to changing environmental and abundance conditions. In neither case does there appear to be significant species competition, due to both geographical and environmental separations, respectively.

The study of Greenland halibut by-catches in the roundnose grenadier fishery by Chumakov and Savvatimsky (1983, 1984) has pointed out the importance of observational scales. Their study indicated by-catches varied with geographic area (latitude), season and year. By-catches were higher in northern areas, moving south to north from NAFO Div. 3K to Subarea 0. Seasonally, by-catches were higher during the summer/autumn period due to the migration of grenadier up the slope with warmer waters. Finally, the annual increases in by-catches during recent years appeared to be due to higher water temperatures at depths normally occupied by Greenland halibut and to population increases in Greenland halibut.

The by-catch of Greenland halibut averaged about 50% and ranged as high as 80% over the years 1970-83 (Chumakov and Savvatimsky 1983, 1984). Canadian research trawl data indicated by-catches ranging from 20 to 60% in Div. 2GH3K (Bowering 1983). The problem stated for this fishery is that by-catch restrictions for Greenland halibut (<10%) has limited catches for grenadier below projected levels. The solution appears to be a management one - allow higher by-catches of Greenland halibut and direct fishing for grenadier into those areas where relative by-catches are lowest. It is detailed information on species habits and movements that will provide for knowledgeable management choices that are effective.

Underlying all of these studies, though seldom explicitly addressed, are the kinds and quality of data available with which to carry out these analyses. These often involve lack of distributional information, both seasonally and for different years. More directly they involve lack of adequate numerical data with which to test the significance of by-catch data. This would involve statistical tests for properly controlled experiments but also adequate quantitative data to reliably express fractions caught. In other words, it may not always be necessary to demonstrate a statistical difference but more reliable estimates of species abundances and occurrences are necessary to measure associations and by-catches with a higher degree of confidence. For example, while the study of cod by-catches in capelin nets (Stevenson 1984) indicated negligible proportions were caught compared to the size of the 2J3KL cod stock complex effects on local populations may be much more pronounced. In addition, estimates of young cod caught and subsequently discarded alive versus dead were based on visual observations and this relied heavily on the experience of the observer. Standardization of observer estimates was not done nor are the 'quantitative' estimates verifiable without comparison to the observers used in this study.

Possibly the best study examining the importance of by-catches was snow crab caught in the cod gillnet fishery (Miller and Hoyles 1973). This was a specific study set up and carried out over two years. It demonstrated crab by-catch, which was discarded with approximately 40% mortality, represented ~ 60% of the landed crab catch and 20% of the estimated MSY for snow crab. This problem was perpetrated by the increased use of cod gill nets during the period 1961-72. Additionally, this by-catch was largely commercial sized crab and therefore represented a direct, and substantial, loss to the crab fishery. The value of this study lay not simply in quantifying a substantial and significant by-catch but also in proferring management solutions to the problem. Here the most promising solution was a technological one suggesting the development of a gillnet that was suspended ~ 1 ft off the bottom and therefore did not catch snow crabs. Thus, the solution was gear development that would make the cod gillnet fishery more 'directed'.

All but one or two of these studies can be considered as management responses to an observed situation. As such it is an approach to fisheries management that responds to situations or problems as they arise. No consideration has been given to questions such as changing production of species assemblages with changing relative abundance of species (cf. Tyler et al. 1982) or stock re-building, especially for pelagic species. The conclusion is that generally by-catch is not a problem, and when it is changing fishing strategies will reduce it to acceptable levels. We think this can be improved

upon both in ...e kinds and quality of data that are used to assess by-catch and in the range of possible solutions (management options) that would involve biological, technological and environmental considerations.

As with biological interactions, studies reported here have been summarized in the annotated bibliography (Appendix 1). From these observations management concerns relating to species by-catch can be summarized:

1. By-catch is not specific to gear type or area. By-catches have been reported for trawls, trap nets and gill nets and for both the inshore and offshore fisheries. In other words, by-catch appears to be an ubiquitous problem to a management approach based on directed, single species fishing effort.

2. The degree of species overlap and how this varies seasonally and between years is the most important factor determining the degree of by-catch for historical fisheries. These distributions can be affected by environmental conditions and changing species abundances.

3. Changing fishing strategies have a profound influence on the degree of species by-catch. This will occur as a result of changes in gear technology, an increased fishery, change of location of a fishery or development of a new fishery.

4. The management approach so far has been one of response to a problem as it arises. This can take the form of changing effort, setting by-catch restrictions, closing areas or modifying the fishing gear. In no case has anticipation of by-catch been built into the management process for species in this region. It should be noted, however, ICNAF imposed a two-tier catch quota system during 1974-77 in Subareas 5 and 6 based on projected by-catch levels and anticipated biological interactions (O'Boyle this volume).

5. By-catch studies mostly have been limited to two species, although this appears to be a result of the single species management approach. Simple ratios of by-catch have formed the basic analytical tool. In only one case (Brodie 1981) has regulation or management of three or more species, simultaneously, been attempted. In this way 'multispecies' often reduces to 'bi-species'.

6. In most cases data quality and quantity are lacking from which to draw clear conclusions, test statistical differences or even make recommendations with a degree of confidence. In most cases the observational scales (time/space) were too coarse. Only in two cases were specific data collection programs set-up to assess by-catch problems. Future progress in effective management involving technological interactions inevitably will require more specific studies improving data quality.

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Discussion Period

Silvert: There is one thing I found somewhat disturbing. It is that in discussing the predator-prey interactions you rely very heavily on correlations between predator and prey. However, when you look at even the simplest food chain models they do not predict correlations between predator and prey. They predict the correlations will skip a trophic level. For example you get significant correlations between primary biomass and carnivore levels and you get structural changes in the intermediate herbivore level. Furthermore, this is supported by studies carried out by limnologists ir reservoirs where they may introduce fish to the system and find that the correlation skips the intermediate level. So the method of looking for predator-prey correlations does not have a strong theoretical basis. It does not appear to be sufficient to simply look for correlations, what is necessary is to put together a fairly complete model of the ecosystem and ask, what does that predict and evaluate interactions within the context of an ecosystem model.

Lilly: Are you saying that there are far more subtle effects than the direct interaction of predator and prey?

Silvert: No, what I am saying is that in a trophic model the steady state response of a change at one level skips a level. If you increase primary production there is a short-term surge in herbivore production but carnivores adapt and the herbivores return to their previous level; then production increases but biomass does not. If you want to look at the system from a dynamical point of view then the time scale of observation is critical. By driving exactly the same model at different rates you could get either a positive or a negative correlation. Furthermore, in saying that you expect a direct relationship between cod and capelin abundance in the system you are assuming that there are no other pathways in the system.

Lilly: In fact we did look at other pathways, in terms of changes in predation levels on other prey in the system. However, we have the problem of not being able to identify changes in abundances of those other prey. In relation to your earlier point one would think that the logical place to start would be with the direct interaction of predator and prey, then to look at other possible interaction effects.

Silvert: Yet if the models do not predict a correlation, why look for it?

Lilly: There is a lot of literature which predicts direct effects between predator and prey in both directions.

Kerr: This would be expected in the non-equilibrium situation, and the cod-capelin system is patently non-equilibrium.

Mohn: You showed a dramatic change in stomach fullness of cod from year to year. Does it express itself in the somatic growth?

Lilly: We have some data with which to look at that question, but have not analysed it yet.

Table 1. Summary of predator-prey interactions identified in marine waters of the Newfoundland Region. Reference numbers refer to numbers in Appendix 1.

Prey	Predator	References: Dynamics	References: Distribution
A. Influence of prey on predator			
Capelin	Cod	1,10	1,10
	Salmon	10,12	
	Seabirds	9,10	
	Baleen whales		11,16
	Harp seal	2	
Redfish	Cod	17	
Squid	Pilot whale		24

B. Influence of predator on prey

Predator	Prey	References
Predation primarily on pre-recruits		
Squid	Cod, capelin, herring	11,14,15
Cod	Redfish	18,20
Cod	Snow crab	19,22
Cod, Greenland halibut	Cod, Greenland halibut, Am. plaice	6,18,19,22
Predation on both pre-recruits and recruited individuals		
Cod, Greenland halibut, seabirds, marine mammals	Capelin	2,9,19,23 26,30,32,35
Cod	Sand lance	22,34
Cod, Greenland halibut	Arctic cod	6,19
Cod, Greenland halibut	Shrimp	6,19
Cod, pilot whale	Squid	21,24

Table 2. Summary of reported by-catch studies in the Newfoundland Region. Reference numbers refer to numbers in Appendix 1.

By-catch study	Interaction	Source
Greenland halibut in roundnose grenadier fishery (Subareas 0, 2 and Division 3K)	Yes	5,13
Snow crabs caught in cod gillnet fishery (Subarea 3 inshore)	Yes	25
Yellowtail flounder in American plaice fishery (Divisions 3LNO)	No (Div. 3L) Yes (Div. 3NO)	8
Witch flounder in cod, redfish and American plaice fishery (Divisions 2J, 3K, 3Ps)	No	3,4
Cod, redfish, American plaice, witch and yellowtail fisheries on the Grand Bank (Divisions 3LNO)	No	7
Juvenile cod in capelin trap fishery (Division 3L)	No	31
Giant and Iceland scallops in St. Pierre scallop fishery (Division 3Ps)	No	27
Baleen whales and harbour porpoise in cod trap fishery and salmon and cod gillnet fisheries (Subareas 0, 3 and Division 2J) inshore	Yes	16,30
Alcids (common murre, Atlantic puffins) caught in fishing nets (Subarea 3)	Yes	29

APPENDIX 1

1. Akenhead, S. A., J. Carscadden, H. Lear, G. R. Lilly, and R. Wells. 1982. Cod-capelin interactions off northeast Newfoundland and Labrador, p. 141-148. In M. C. Mercer [ed.] Multispecies approaches to fisheries management advice. Can. Spec. Publ. Fish. Aquat. Sci. 59.

This paper reviewed the distribution, fishery, and population trends for 2J3KL cod and 2+3K capelin, and described briefly the seasonal patterns of predation by cod on capelin in 2J3KL. The effects of fluctuations in capelin biomass on cod growth and the inshore cod fishery were tested by correlation analysis. There were no statistically significant relationships between cod growth and capelin biomass, cod biomass, and water temperature. The relationship between index of availability of cod to traps and one of the two available indices of mature capelin biomass was significant.

There were many uncertainties in the available data. Differences between the two available estimates of capelin abundance could not be resolved; annual determinations of cod length-at-age were made during the feeding and growth period rather than at the end; differences in indices of growth of cod from different areas could not be resolved; the index of inshore cod migration was catch rather than catch per effort; and temperatures were available from only one site.

The authors cautioned against acceptance of the findings because of their strong reservations about the appropriatness of the data. However, they noted that production of Atlantic cod may not be strongly linked to capelin, and that other prey might provide adequate forage during periods of low capelin abundance. With respect to the cod inshore migration, they suggested that site-specific studies be conducted, with emphasis on the local abundance and movement of both cod and capelin in relation to changes in oceanography.

2. Bowen, W. D. 1981. Harp seals and their foods: How do they interact? NAFO SCR Doc. 81/154, Ser. No. N461. 8 p.

In press. Harp seal feeding and interactions with commercial fisheries in the Northwest Atlantic. In J. R. Beddington, R.J.H. Beverton, and D. M. Lavigne [ed.] Interactions between marine mammals and fisheries. George Allen and Unwin Publishers, London.

The second paper expands themes developed in the first.

The author noted the concern about the possible impact of reduced capelin abundance on marine mammals in general and harp seals in particular. He reviewed information on the food of harp seals and concluded that the relative proportions and seasonal and annual variations of different foods in the diet was poorly known. However, harp seals do feed on a variety of fish and crustacea, so they might feed on other prey when capelin stocks are reduced. It was suggested that the impact of fluctuating prey populations be studied by indirect methods, such as monitoring condition, mean age at sexual maturity, and pregnancy rate.

The major drawback of such an approach is that it does not address the casual mechanisms of the interactions. It was also concluded that the annual food consumption by the seal population could not be calculated with precision because of uncertainty about diet, feeding rate, and population size. Estimation of the potential loss in yield to commercial fisheries due to predation by seals is further complicated by uncertainity regarding relationships with other species, both competitors and prey.

3. Bowering, W. R. 1981a. Witch flounder of St. Pierre Bank - a by-catch fishery. CAFSAC Res. Doc. 81/54. 12 p.

This study indicated that witch flounder catches are essentially by-catches in the cod, redfish and American plaice fisheries in NAFO Division 3Ps. Commercial landings (1963-80) indicated witch flounder by-catches were low (1000 t annually) while research biomass surveys (1972-81) indicated abundance has changed little. The fishery has removed fish 12 yr age but has taken an average of about 1/3 of the total allowable catch annually since 1974. It was concluded there was no significant by-catch of witch affecting the stock size.

4. **Bowering, W. R. 1982. An examination of the witch by-catch fishery in NAFO Divisions 2J and 3KL. CAFSAC Res. Doc. 82/28. 22 p.**

This study examined the fishery for witch flounder in NAFO Divisions 2J3KL in an attempt to assess the stock biomass and significance of the by-catch. This was an update of an analysis done the previous year (Bowering 1981b). Commercial landings from 1967-81 indicated a decline to around 3000 t as the fishery moved from a directed gillnet (inshore) and otter trawl fishery (offshore) to one of largely by-catch in recent years. Witch occurs as a by-catch in cod, American plaice and particularly in Greenland halibut and redfish fisheries where the by-catch can be "significant" in terms of proportion of catch. Due to low landings, the by-catch nature of the witch fishery and a short time series of research data it was difficult to assess mortality and biomass estimates for the "stock area", apparently made up of at least three separate breeding stocks. Insufficient data on fishing effort, recruitment or natural mortality limited analysis and interpretation. No definite conclusions were reached.

5. **Bowering, W. R. 1983. By-catch levels of Greenland halibut in the roundnose grenadier directed fishery of NAFO Subareas 2+3. NAFO SCR Doc. 83/28, Ser. No. N680. 4 p.**

Reduced catches of roundnose grenadier due to a 10% by-catch restriction for Greenland halibut lead to an examination of data available from the Canadian Foreign Observer Program and research vessel data. These data covered the years 1981-82 and 1977-82, respectively. By-catch of Greenland halibut in the commercial data ranged from 5 to 41% and in the research trawl data from 20 to 60%. It was concluded best catches of grenadier occurred in depths > 1000 m and in more northerly areas. There was no assessment of the signifiance of this by-catch on Greenland halibut stocks.

6. **Bowering, W. R., D. G. Parsons, and G. R. Lilly. 1984. Predation on shrimp (Pandalus borealis) by Greenland halibut (Reinhardtius hippoglossoides) and Atlantic cod (Gadus morhua) off Labrador. ICES C.M.1984/G:54. 30 p.**

This paper described the prey spectrum and stomach fullness of Greenland halibut and cod in Hopedale and Cartwright Channels off Labrador, and made gross estimates of the annual consumption of shrimp (Pandalus borealis) by these predators.

Shrimp and small Greenland halibut were the major prey of both predators. Both predators fed on the full size range of shrimp, so they not only compete with the fishery for large shrimp but also reduce recruitment to the fishable population. In July 1981 Greenland halibut were 100 times more abundant than Atlantic cod in Hopedale Channel and 25 times more abundant in the more southerly Cartwright Channel. However, the Atlantic cod were of a larger average size and at a given body length had more shrimp in their stomachs. Consequently, Atlantic cod were significant predators in Hopedale Channel but not as important as Greenland halibut, whereas in Cartwright Channel Atlantic cod appeared to be the more important predator. Gross estimates of potential shrimp consumption by the two predators in 1981 indicated that predation mortality might be low (<10%) relative to estimates of minimum shrimp biomass in Hopedale Channel, but greater (about 60%) in Cartwright Channel.

The estimate of shrimp removal by the two predators might be improved with additional information on the following: seasonal and annual variation in abundance of predators and prey; rate of evacuation of shrimp from predator stomachs at low temperature (2-4°C); functional feeding response; prey preference. In addition, mortality due to predation should be compared with shrimp production, not shrimp biomass, so an assessment of the impact of predation also requires information on shrimp growth rate and post-larval recruitment.

7. **Brodie, W. B. 1981. By-catches of five Grand Bank groundfish fisheries. CAFSAC Res. Doc. 81/68. 18 p.**

This study examined the degree of by-catch in five directed fisheries on the Grand Bank (3LNO), to estimate the directed catch in 1981, having accounted for by-catch using a linear programming model. By-catch ratios were determined for cod, redfish, American plaice, witch and yellowtail from catch data for the years 1976-79. The results indicated that estimated directed catches for 1981 varied with the relative proportions of annual catches (1976-79), but that by-catch was always sufficiently high to account for total permitted catch for each of the five species. The highest by-catch ratios were consistently for American plaice in each of the cod, witch and yellowtail directed fisheries. In this regard these fisheries are more correctly classified as mixed fisheries, as opposed to directed. Due to the inter-annual variability in the catch data it would be difficult to use these results in projecting by-catch regulations.

8. **Brodie, W. B. 1983. By-catches of yellowtail in the American plaice fishery in NAFO Divisions 3L, 3N and 3O. CAFSAC Res. Doc. 83/72. 54 p.**

This study examined the trend in by-catches of yellowtail and American plaice in their respective directed fisheries on the Grand Bank (3LNO). Data examined included Canadian commercial trawler log books (1974-81), observer information from Canadian TC 5 trawlers (1979-81) and research vessel surveys (1971-82). By-catches of yellowtail in the directed fishery for American plaice had decreased during the period 1977-81. This was shown to be due to *a change in fishery strategy that favoured* catches of American plaice over yellowtail. Due to distributional differences of these species on the Grand Banks the fishery was directed towards plaice (Division 3L) and away from yellowtail (Division 3N). Considering the decreased by-catches and that TAC's for yellowtail were not being filled, it was concluded there was no by-catch problem, as such. Finally, considering yellowtail by-catch, as a percent of directed yellowtail catch, ranged from 20 to 106% and averaged 47% over 8 years it is most feasible to consider this a mixed, versus single-species, fishery. This is particularly true for Div. 3N.

9. **Brown, R.G.B., and D. N. Nettleship. 1984. Capelin and seabirds in the northwest Atlantic, p. 184-194. In D. N. Nettleship, G. A. Sanger and P. F. Springer [ed.] Marine birds: their feeding ecology and commercial fisheries relationships. Proc. Pacific Seabird Group Symposium, Seattle, Washington, 6-8 January 1982. Can. Wildl. Serv. Spec. Publ.**

This paper reviewed the feeding habits and population sizes of seabirds off Newfoundland and Labrador and estimated the annual consumption of capelin by seabirds to be about 250,000t.

The importance of capelin to seabirds was demonstrated by reduced breeding success of puffins when capelin were scarce or absent during the chick-rearing stage. Capelin were the principal prey brought to puffin chicks on Great Island, Witless Bay, in 1968-69, but the young birds were fed mainly on small gadids in 1981. In 1981, chicks received an average of only 13.47g/day (against 44.64 in 1968-69), fledged at an average body weight of 217.0g (against 261.6), and fledging success was only 45.0% (against 60.3%). It was concluded that capelin is an essential part of the diet and that there is no alternative prey, of comparable size, abundance, and nutritional value, available within range of Witless Bay.

It was acknowledged that it is not clear to what extent the scarcity of capelin off Witless Bay in 1981 was an effect of the capelin fishery. However, it was suggested that any serious decline in the capelin stocks would have adverse effects on the principal predators.

(See Carscadden (1983) for a more detailed review.)

10. **Carscadden, J. E. 1983. Capelin as a forage species: a review of selected studies. NAFO SCR Doc. 83/72, Ser. No. N738. 7 p.**

This paper reviewed those studies which examined available data to determine whether changes in capelin abundance effect detectable changes in the dynamics or behaviour of cod, salmon, and seabirds.

Of particular interest is the review of the capelin-seabird study by Brown and Nettleship (1984). For example, with respect to the puffin study at Witless Bay, Carscadden pointed out that the estimates of chick feeding rate were based on only three days each year during a period near the end of fledging and well after the peak of capelin spawning. In addition, the observation that the fledging rate declined from 60.3% in 1968-69 to 45.0% in 1981 is misleading, because in fact the fledging success was 43.2% in 1968 and 66.9% in 1969. The low fledging success in 1968, before initiation of a capelin fishery, demonstrates that factors other than a capelin fishery influence puffin breeding success.

Carscadden noted that, in each of the capelin-predator relationships, information on the predator's diet is limited spatially and temporally, and thus there is much uncertainty involved in determining the importance of capelin in the diet. Because both predator and prey fluctuate in abundance, feeding studies should be conducted over a number of years.

The author noted that data required for addressing specific hypotheses regarding multispecies interactions were usually found to be non-existent or unsuitable. He suggested that such interactions be the subject of directed, well-planned, independent studies.

11. **Carscadden, J. E. 1984. Capelin in the Northwest Atlantic, p. 170-183. In D. N. Nettleship, G. A. Sanger, and P. F. Springer [ed.] Marine birds: their feeding ecology and commercial fisheries relationships. Proceedings of the Pacific Seabird Group Symposium, Seattle, Washington, 6-8 January, 1982.**

This paper briefly reviewed studies of the relationships between capelin and squid, cod, salmon, whales, and seabirds.

The author pointed out that the negative correlation between squid abundance and year-class success in capelin, reported by Dawe et. al. 1981, may have disappeared with the addition of data for 1979, since in that year squid were very abundant and the year-class strength in capelin was very high.

The author also mentioned the correlation noted between the increase in whale collisions with inshore fishing gear in Newfoundland and the decline in capelin abundance in the late 1970's. It has been postulated that decreased abundance of capelin offshore would result in increased migration of whales to inshore waters where capelin would be concentrated for spawning. An assessment of the data relevant to this relationship has not yet been published.

12. **Carscadden, J. E., and D. G. Reddin. 1982. Salmon-capelin interactions. Here today, gone tomorrow. CAFSAC Res. Doc. 82/23. 10 p.**

The poor sea survival of the 1977 smolt-class of salmon during its first winter at sea was attributed in the popular press to a decline in capelin abundance due to overfishing. In this and an earlier paper (Reddin and Carscadden 1981. CAFSAC Res. Doc. 81/2), the authors of the present paper reviewed information on salmon distribution and feeding and tested with correlation analysis many possible hypotheses regarding a negative relationship between capelin abundance and salmon survival. Approximately 19% of the biologically possible relationships between post-smolt salmon, adult salmon, and capelin were statistically significant. When these relationships were recalculated using more recent and revised information, all were no longer significant.

13. **Chumakov, A. K., and P. I. Savvatimsky. 1984. Roundnose grenadier-Greenland halibut ratio in bottom trawl catches taken in NAFO area in 1970-1983. NAFO SCR Doc. 84/37. Ser. No. N822. 15 p.**

This study examined the by-catch of Greenland halibut in the roundnose grenadier fishery of NAFO Subarea 2 and Division 3K. This was prompted by increased Greenland halibut by-catches in recent years and limitations in grenadier catches due to by-catch regulations. From commercial ships (1973-1981) and research ships (1970-1983) Greenland halibut by-catches averaged 40.9% and 48.4%, respectively. By-catch was found to vary with: latitude, season and years. In general, by-catch was highest in northern areas during the summer-autumn period and had increased in recent years. Increased by-catches during recent years was thought to be due to increased Greenland halibut abundance and cooler water temperatures in the 400-1100 m depth range. There was no significant increase in halibut by-catch with increasing depth in the 400-1100 m depth range. It was concluded that by-catch regulations were "impeding" the catches of roundnose grenadier.

14. **Dawe, E. G., G. R. Lilly, and H. J. Drew. 1983. Predation by short-finned squid (Illex illecebrosus) in Newfoundland inshore waters. NAFO SCR Doc. 83/74, Ser. No. N740. 16 p.**

This paper provided data on prey spectrum and stomach fullness of squid in inshore Newfoundland waters in 1980 and 1981. Major prey were fish and squid. Fish otoliths were almost entirely from gadoids, presumably Atlantic cod. Correlation analysis was used to test whether there was a negative relationship between inshore squid abundance and year-class strength in the 2J+3KL, 3NO, and 3Ps cod stocks, assuming that squid prey on cod during the year of hatching, during the year after hatching, or during the two years combined. No significant relationships were found. Most of the problems identified during an earlier study (Dawe et.al. 1981) were again noted.

15. **Dawe, E. G., G. R. Lilly, and J. A. Moores. 1981. An examination of the influence of squid (Illex illecebrosus) on recruitment in several finfish stocks off eastern Newfoundland and Labrador. NAFO SCR Doc. 81/25, Ser. No. N304. 14 p.**

Because squid migrate to Newfoundland waters in very large numbers in some years, feed on fish, and grow very rapidly, it has been speculated that they might significantly reduce the

abundance of some commercial fish species. The annual abundance of squid at Newfoundland fluctuates dramatically, so the authors hypothesized that predation by squid on fish juveniles would be manifested in an inverse relationship between squid abundance and year-class strength.

Correlation analysis was used to test the hypothesis that there was a negative correlation between squid abundance and year-class strength in one cod stock, 4 herring stocks, and 2 capelin stocks, assuming that squid prey on 0-group or 0-group plus 1-group juveniles. A significant relationship was found only for Div. 2+3K capelin assuming predation on age-groups 0+1. (This correlation may not be significant if data from more recent years are added (Carscadden 1984)).

The authors noted several problems with the analysis. For example, there is no objective index of squid abundance, the actual consumption of any given prey species in a particular year could not be calculated, and the ages of the prey were not known. The major problem was the lack of population estimates for pre-recruits, so squid abundance could be compared only with the year-class strength of the youngest age-group available from cohort anlaysis, rather than with mortality rates.

16. **Lien, J., and S. Gray. 1980. Net loss. Nature Canada. 9: 5-9.**

A marked increase in whale entrapments (by-catch) in inshore fishery gear during 1979-80 prompted a study of its effects on fishermen. A program was set up with fishermen to collect information on whale entrapments and to develop a whale release program. The increase in whale entrapments was concluded to be a result of reduced capelin biomass offshore and increased inshore fishing effort. A program which developed and trained fishermen to release trapped whales resulted in less gear damage and fishing down-time. In addition, development of low-frequency underwater alarms was undertaken to reduce collisions. However, it was felt the first step in reducing collisions was to enhance recovery of capelin stocks and thereby redistribute the whales offshore. This program has concentrated on understanding the nature of the problem, particularly as this affects fishermen, and providing solutions. As such the interaction is a man-whale interaction.

17. **Lilly, G. R. 1980. Year-class strength of redfish and growth of cod on Flemish Cap. ICNAF Sel. Pap. 6: 35-39.**

This paper presented the hypothesis that the growth rate of cod on Flemish Cap is dependent on the abundance of prey of a suitable size, but provided no new data.

It was noted that the Flemish Cap lacks large resident populations of fish species whose adults are of a size suitable as prey for adult cod. The growth rate of cod increased in the 1960's and early 1970's compared with the 1940's and 1950's. This increase coincided with the appearance of highly successful year-classes of redfish. It was hypothesized that juvenile redfish provided the energetically-favourable forage needed for high growth rate of adult cod.

Significant changes in prey composition occurred between 1978 and 1983 (Lilly 1983) but there has been no examination of annual changes in feeding and growth to determine whether a close link between the two can be demonstrated. The rapid decline in cod abundance during the late 1970's will complicate the analysis.

18. **Lilly, G. R. 1983. The food of cod on Flemish Cap in winter 1983. NAFO SCR Doc. 83/65, Ser. No. N726. 7 p.**

This paper presented data on predation by cod on smaller cod and small redfish on the Flemish Cap in winter 1983. The cod were preying on the three most recent year-classes of redfish and the two most recent year-classes of cod. The age and number of prey increased with predator size.

It was noted that estimation of the number of individuals of each species consumed by the cod could not be reliably estimated because of inadequate seasonal information on cod feeding, particularly in the autumn when 0-group juveniles are first becoming available to demersal predators. However, even if removal rates due to cod predation could be reliably estimated, mortality rates could not be calculated without independent measures of abundance.

19. Lilly, G. R. 1984a. Annual variability in the diet of Atlantic cod (Gadus morhua L.) off southern Labrador and northeast Newfoundland (Div. 2J+3K) in autumn, 1977-82. NAFO SCR Doc. 84/79, Ser. No. N868. 12 p.

1984b. Predation by Atlantic cod on shrimp and crabs off northeastern Newfoundland in autumn of 1977-82. ICES C.M.1984/G:53. 25 p.

These two papers described the prey spectrum and stomach fullness of cod in Div. 2J+3K during the autumns of 1977-82 to determine whether there had been annual variability in feeding associated with the dramatic changes in capelin abundance during this period. The NAFO document concentrated on capelin whereas the ICES document concentrated on shrimp (Pandalus borealis) and crabs (Chionoectes opilio).

The major prey were capelin, shrimp, crabs, hyperiid amphipods and Arctic cod. The rate of predaton by Atlantic cod on capelin did vary with capelin abundance. During the period of low capelin abundance in the late 1970's Atlantic cod did not compensate for reduced predation on capelin by preying more intensively on shrimp, crabs, or other benthic invertebrates, but there was increased predation on Arctic cod and hyperiids. This increased predation on alternate prey only partially compensated for the reduction in predation on capelin.

Cod growth data collected during the surveys have not yet been examined to determine whether growth responds to changes in the availability of capelin.

20. Lilly, G. R., and C. A. Gavaris. 1982. Distribution and year-class strength of juvenile redfish, Sebastes sp., on Flemish Cap in the winters of 1978-82. J. Northw. Atl. Fish. Sci. 3: 115-122.

This paper presented information on relative abundance of juvenile redfish in trawl catches and cod stomachs during January-February of 1978 to 1982. The 1977 and 1979 year-classes were very weak as 1-year-olds. The 1978 year-class, which appeared to be abundant in cod stomachs and moderately abundant in trawl catches in January-February, 1979, was very weak in subsequent years, indicating that mortality of juvenile redfish can be high. The intense predation on this year-class by cod in February, March, and May of 1979 is evidence that predation may have been a significant component of the mortality during 1979, although other factors such as inadequate food supply and unfavourable environmental conditions may have contributed.

The 1980 year-class was abundant as 1-year-olds but, unlike the 1978 year-class, it survived in large numbers as 2-year-olds. If predation is the major cause of mortality in juvenile redfish, success of the 1980 year-class may have been due to 1) their survival in sufficiently large numbers from the larval stage as to swamp the ingestive capacity of predators, or 2) decreased predation on early juveniles as a result of a significant decline in cod abundance and the appearance of a strong succeeding year-class as alternate prey. The authors noted that it is unlikely that such possibilities can be distinguished without estimates of absolute abundance, especially at the early demersal stage, and seasonal information on removals due to predation.

21. Lilly, G. R., and D. R. Osborne. 1984. Predation by Atlantic cod (Gadus morhua) on short-finned squid (Illex illecebrosus) off eastern Newfoundland and in the northeastern Gulf of St. Lawrence. NAFO SCR Doc. 84/108, Ser. No. N905. 16 p.

This paper presented information to show that in years of high abundance the short-finned squid was a common prey of cod in summer and autumn in both inshore and offshore waters of eastern Newfoundland. The frequency of occurence of squid in cod stomachs and the number of squid per stomach increased with cod length. The intensity of predaton by cod on squid was low compared with peak predation on capelin and sand lance. Nevertheless, there is evidence that squid in years of high abundance provide an increase in total food availability, especially for large cod.

22. Lilly, G. R., and J. C. Rice. 1983. Food of Atlantic cod (Gadus morhua) on the northern Grand Bank in spring. NAFO SCR Doc. 83/87, Ser. No. N754. 35 p.

This paper presented an analysis of the stomach contents of cod collected on the northern Grand Bank in May-June, 1979. The major prey were sand lance, capelin, snow crab, a toad crab, and an euphausiid. The importance of capelin (15% by weight) was low compared with the results of other studies off eastern Newfoundland and Labrador. Variability in stomach contents was

attributable in part to differences in distribution of the various prey and to a gradual change in diet with increasing cod length. However, there was no strong preference for any of the major prey types.

The paper introduced the use of cluster analysis in conjunction with simulations of clusterings based on various neutral models to test specific hypotheses about feeding behaviour.

23. Lilly, G. R., R. Wells, and J. Carscadden. 1981. Estimates of the possible consumption of capelin by the cod stocks in Div. 2J+3KL and 3NO. NAFO SCR Doc. 81/8, Ser. No N272. 9 p.

This paper briefly reviewed the seasonal pattern of feeding by cod on capelin in the area from southern Labrador to the southern Grand Bank, and described previous estimates of the consumption of capelin by cod.

Two methods were used to estimate the probable consumption of capelin by cod in 1981. The first assumed that published values of the annual consumption of capelin per unit biomass of cod could be applied to the average biomass of cod projected for 1981. The second method involved estimation of the production and total food consumption by cod in 1980-81. A portion of the total consumption was assigned to capelin on the basis of previous observations of the occurrence of capelin in cod stomachs. Estimates ranged from 1155 to 4391 X 10^3 t.

It was noted that inadequate information was available for several steps in the estimations. This included spatial, seasonal, and annual variability in cod feeding; gastric evacuation rate in cod; annual production by cod, particularly the gonadal part; and metabolic rate of cod at low temperature.

24. Mercer, M. C. 1975. Modified Leslie-DeLury population models of the long-finned pilot whale (Globicephala melaena) and annual production of the short-finned squid (Illex illecebrosus) based upon their interaction at Newfoundland. J. Fish. Res. Board Can. 32: 1145-1154.

In Newfoundland inshore waters the long-finned pilot whale feeds almost exclusively on short-finned squid. Seasonal occurrences of the two species inshore are nearly coincidental, and annual landings of the two species in Newfoundland between 1952 and 1972 were positively correlated. These observations support the hypothesis that the whales follow the squid. Assuming a mean body weight (B.W.) of 830 kg, a daily feeding rate of 4-6% B.W., a period of feeding on squid of 100-365 days, and population estimates of 50,000-80,000 individuals, the annual consumption of squid by pilot whales would be between 0.17 and 1.45 million tons.

25. Miller, R. J., and J. R. Hoyles. 1973. Loss of commercial snow crabs to cod gillnets in Newfoundland. Fish. Res. Board Can. Tech. Rep. No. 429: 21 p.

This study was undertaken to assess the magnitude of the annual loss of snow crabs to the gillnet fishery for flatfish off NE Newfoundland and to identify areas of the greatest concern. Data were collected during 1972 and 1973 and included: laboratory measurements of survival following injury; interviews with fishermen; commercial observations aboard crab boats; and, CPUE (landings/boat/yr). This was an excellent, objective study examining technological interactions within a fishery which produced clear results, conclusions and possible solutions to the problem. Results indicated crab loss due to by-catch in the gillnet fishery was significant at 2 x 10^6 lbs/yr compared to 1972 landings of 3.3 x 10^6 lbs (61%) and a MSY of 9 x 10^6 lbs (22%). Among suggestions for alleviating the problem, development of a gill net that fishes ~ 1 ft off the bottom to reduce crab by-catches was felt to hold the most promise. Changing fishing strategies with changing densities of crab populations were also discussed.

26. Minet, J. P., and J. B. Perodou. 1978. Predation of cod, Gadus morhua, on capelin, Mallotus villosus, off eastern Newfoundland and in the Gulf of St. Lawrence. ICNAF Res. Bull. 13: 11-20.

This paper presented data on the food of cod in Div. 2J,3KL in summer and winter.

Assuming that cod ate in discrete meals, the mean weight of capelin in a meal was estimated from empirical data. The number of meals of capelin per year was estimated from published information on gastric evacuation rate. From this information it was calculated that the average cod consumes, at a minimum, 0.76 to 1.27 times its body weight in capelin annually. Thus, in

1965-69, when the cod biomass averaged 2.6 million tons, the cod ingested 2.0-3.4 million tons, annually. (See Lilly et al. (1981) for a more detailed discussion of the assumptions in these calculations).

27. **Naidu, K. S., and J. T. Anderson. 1984. Aspects of scallop recruitment on St. Pierre Bank in relation to oceanography and implications for resource management. CAFSAC Res. Doc. 84/29. 15 p.**

This study considered various aspects of scallop recruitment on St. Pierre Bank in relation to management strategies. The scallop fishery is largely directed for the giant (or sea) scallop which in turn is dependent on successful recruitment. The co-occurrence of giant and Icelandic scallop on the NW portion of St. Pierre Bank and the nature of recruitment (internal vs external) raises several questions regarding competition and resource partitioning between these species. By-catch, as such, is not considered as a problem.

28. **Perkins, J. S., and P. C. Beamish. 1979. Net entanglements of baleen whales in the inshore fishery of Newfoundland. J. Fish. Res. Board Can. 36: 521-528.**

Whale entanglements around Newfoundland were monitored from 1973-77. Estimates of both entanglement and death indicated approximately 3 minke, 3 humpback and <1 finback whale become entrapped yearly and that of these approximately 2 minke, 2 humpback and <1 finback die every year as a result. The general nature of the problem was outlined in terms of the timing and locations including the association of whales, capelin and cod in inshore waters. The need for preventative measures was outlined with net alarms probably being the most effective solution. Finally, it was pointed out this was very much a whale-fishermen interaction problem. Data to assess the impact of mortality on whale populations was not available.

29. **Piatt, J. F., D. N. Nettleship, and W. Threlfall. 1984. Net mortality of common murres and Atlantic puffins in Newfoundland, 1951-81. In D. N. Nettleship, G. A. Sander, and P. F. Springer [ed.] Marine birds: their feeding ecology and commercial fisheries relationships. Minister of Supply and Services Canada 1984. Cat. No. CW66-65/1984. ISBN0-662-13311-0.**

An initial attempt was made to estimate mortality of common murres and Atlantic puffins as by-catches in fishing nets. Their calculations indicated net mortality around the Witless Bay bird sanctuary may have ranged from 3-20% of the local breeding population of common murres. By-catch of adult Atlantic puffins was low, never exceeding 1.6% of the breeding population. However, there are a number of assumptions and biases inherent in these estimates that were not accounted for and results should be treated as preliminary.

30. **Sergeant, D. E. 1973. Feeding, growth, and productivity of northwest Atlantic harp seals (Pagophilus groenlandicus). J. Fish. Res. Board Can. 30: 17-29.**

1976. The relationship between harp seals and fish populations. ICNAF Res. Doc. 76/125, Serial No. 4011. 5 p.

The first paper presented data on the food of harp seals, demonstrating that the food spectrum is wide. The major prey were small pelagic fish, especially capelin, and both pelagic and benthic crustaceans. An annual total consumption of about 2 million tons in Subarea 1-4 was estimated from information on population size (1.33 million), mean body weight (B.W.) (100 kg), daily ration (5% B.W.), and number of feeding days per year (300). Capelin were estimated to represent 25% of the diet. In the second paper it was noted that a daily ration of 3% B.W. may be more realistic for seals in the wild, so the annual consumption of capelin was revised downward from 0.5 to 0.3 million tons.

31. **Stevenson, S. C., A. D. Murphy, and R. B. Stead. 1984. Report on the cod by-catch in the 1981-83 Newfoundland capelin trap fisheries. Can. Tech. Rep. Fish. Aquat. Sci. 1310: iv + 15 p.**

This study addressed concerns of possible significant by-catches of juvenile cod caught in capelin trap nets. This concern was motivated by increased capelin trap landings in 1979 and 1980 due to an expansion of this inshore fishery. Data were collected during the 1981-83 capelin trap fishing seasons in Trinity and Conception Bays where the majority of the landings occurred. This consisted of observer estimates (visual) of: total weight of fish caught for each species

(cod-capelin); the amount landed; and the amount discarded. In addition, estimates of the amount of fish discarded alive versus dead were also made. While the data are highly subjective the results suggested only insignificant numbers of juvenile fish were being lost as a by-catch of this fishery. While quantities of juvenile cod removed ranged from 4.4 to 8.3 x 10^5 this was estimated to represent only between 0.1-0.2% of estimated recruitment at age 4 to the 2J3KL cod stock complex. Local effects in Trinity and Conception bays may be considerably higher. While the numbers of cod removed ranged up to 831,000 fish, these were mostly 2-3 year-olds and therefore of little commercial value. Low by-catches of cod, in relation to biomass of capelin in trap nets, was also reported by Nakashima (1984).

32. Turuk, T. N. 1978. The feeding of Labrador and Newfoundland cod on capelin. Trudy PINRO 41: 67-73 (Can. Transl. Fish. Aquat. Sci. 4580, 1979).

The author combined estimates of feeding rate, observations on the percentage of the year when cod feed on capelin, observations on frequency of occurrence of capelin in cod stomachs, and estimates of cod biomass, to calculate that cod off Labrador (2GHJ) and on the southern Grand Bank (3NO) respectively consume 3.8 and 6.5 times their biomass in capelin annually.

The source of the feeding rate estimates was not provided, several assumptions are not valid, and there are some discrepancies between the procedures described in the text and the actual calculations performed. (See Lilly et al. (1981) for a more detailed review).

33. Wiles, M. 1968. Possible effects of the harbour seal bounty on codworm infestations of Atlantic cod in the Gulf of St. Lawrence, the Strait of Belle Isle and the Labrador Sea. J. Fish. Res. Board Can. 25: 2749-2753.

This study assessed the effects of a federal government bounty for harbour seals on cod worm (Porrocaecum and Anisakis) infestations of Atlantic cod in the Gulf of St. Lawrence (Divisions 4R,S). It was concluded the effect of bounty kills on infestation was probably small. This was due to: a) a decline in seal kills during the period 1952-66; b) 85% of kills comprised were seals 1 year old in which nematode infestation is small; c) hunting was not "intense" in these areas; d) harbour seals may only account for 2% of all infestations, with harp and grey seals accounting for the rest. No recommendations were made concerning the continuation of the bounty program or for future studies.

34. Winters, G. H. 1983. Analysis of the biological and demographic parameters of the northern sand lance, Ammodytes dubius, from the Newfoundland Grand Bank. Can. J. Fish. Aquat. Sci. 40: 409-419.

The author examined available information on sand lance on the Grand Bank to determine whether there were changes attending the decline in biomass of cod during the 1970's and the substantial reduction in capelin biomass during the late 1970's. An analysis of incidental catches during research bottom-trawl surveys indicated that the abundance of sand lance was relatively stable at a low level up to the late 1960's and gradually increased to a level in the late 1970's which was several times higher than during the 1960's. Mortality rates decreased during the same period. A significant negative correlation was found between sand lance abundance and cod biomass, supporting the hypothesis that reduced predation was responsible for the reduction in mortality and the increase in biomass from the 1960's to the late 1970's.

35. Winters, G. H., and J. E. Carscadden. 1978. Review of capelin ecology and estimation of surplus yield from predator dynamics. ICNAF Res. Bull. 13: 21-30.

This surplus-production model was intended to provide a first estimate of potential long-term annual yield of capelin in Subareas 2 and 3. Since many capelin predators had declined in abundance in the 1950's and 1960's, it was assumed that some capelin previously consumed by predators would be available to a fishery in the 1970's. It was estimated that annual capelin consumption by cod declined from 3.97 to 3.00 million tons, by harp seals from 0.43 to 0.30 millions tons, and by fin whales from 0.36 to 0.25 million tons. The total excess production of capelin released by the decline in abundance of major predators was estimated to be about 1.25 millions tons.

It was noted that these calculations were extremely crude and relied on a number of untested assumptions. (Adapted from review in Carscadden, 1984).

SESSION II:

HARVESTING INTERACTIONS

Review of Methods and Data Used in Analysing Multispecies Fisheries Interactions

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Brodie, W.B. 1984. Review of Methods and Data Used in Analysing Multispecies Fisheries Interactions, p. 55-59. In: R. Mahon [ed.] Towards the Inclusion of Fishery Interactions in Management Advice. Can. Tech. Rep. Fish. Aquat. Sci. No. 1347.

Interpretation of catch data from commercial fisheries which catch two or more species is discussed. Problems with definition of main species data from these fisheries are examined, and in light of these difficulties, uses of observer as well as research vessel survey information are identified. Control of by-catches in multispecies fisheries is identified as an important management tool. Practices to control by-catches have included trip limits, closed seasons, and restricted areas and these measures are evaluated briefly. Methods used previously to analyse multispecies fishery data have included Schaefer models and linear programming exercises. Both these techniques have dealt with the establishment of an overall TAC or MSY for all species in a fishery which is somewhat below the sum of the TAC's or MSY's of the individual species. The linear programming approach used in the Subarea 5 and 6 fisheries in the early 1970's led to the setting of a second-tier quota, which was in fact an overall multispecies TAC.

Introduction

This paper examines data collected from multispecies fisheries and discusses ways in which this data can be used, at a first level, to analyse or interpret these fisheries. The data considered are basic fishery statistics such as catch levels, by-catch rates, etc., and the analyses of multispecies fisheries described here will be those which are limited to these types of data, among which are general production and linear programming models. This paper does not deal with analyses related to identification of multispecies systems (cluster analysis, etc.) nor does it deal with models which require data beyond the complexity of that described above (multispecies VPA's, trophic interactions, bioeconomic models, etc.), although these types of analyses form a significant part of ongoing work on multispecies fisheries systems.

Interpretation of Data From Multispecies Fisheries

With the introduction of total allowable catches (TAC's) in the mid 1970's for many of the fisheries conducted in the northwest Atlantic, came the need for accurate catch statistics to enforce the TAC's and assess stock status. Virtually all TAC's assigned in recent years have been for individual species with some exceptions, notably Scotian Shelf flatfish (Halliday MS 1973; Neilson and Dale MS 1984) and most redfish stocks, where species determination is a problem (Ni 1981). It therefore became necessary to collect the basic fisheries information on a species by species basis whenever possible.

In certain fisheries, where one desired species forms virtually all of the catch, collection and interpretation of data such as catch per day, catch per trip, measurement of effort, etc., is straightforward. However, in fisheries where two or more species are contained in the catch, these statistics become more difficult to calculate on an individual species basis. Dickie (1965) describes the usual data processing system common to fishing vessels required to keep log-books of catch information. It is the eventual summary of such data which leads to problems of interpretation.

Catch and effort data is often categorized, by species, under the heading of main species fishery. This label is usually assigned to the predominant species, by weight, in the catch after summing the totals for a vessel over a period of time, often 1 day or 1 trip. Catch rates and effort data for the main, or directed, species can then be calculated (Lux 1964). These main species catch rates are often used as indicators of stock biomass levels in assessment of stock status (eg. Pitt and Brodie MS 1980). However, Dickie (1965) describes a situation for cod and haddock fisheries on the Scotian Shelf where vessels fished both species and at the end of certain trips it was impossible to distinguish which of the two was the so-called main species. This type of situation is common throughout most multispecies fisheries and represents the major difficulty in dealing with data from these fisheries.

To this point, the multispecies fisheries data described has come from commercial fishing records. Recognizing the limitations of this type of information, it is often possible to utilize data from other sources, primarily observer records and research vessel survey data. Observer information has one advantage in that it is possible to identify a main species sought category from catch records, rather than having to assign a main species based on catch summaries. This type of information is useful in examining changes in catch compositions over time

in certain fisheries (Brodie MS 1983). Research vessel survey results can also be used in identifying trends in catches in terms of species mix, and additionally can be used to examine particular locations within a fishery on a more detailed level, since commercial statistics are usually only available for relatively large areas. In many instances (eg. Sissenwine et al. 1982; Gabriel and Tyler 1980) research vessel data has been used to identify or define multispecies population interactions.

By-Catches and Their Regulation

Often, it is not the catch of the main species (if indeed there is one) which is of primary concern in a multispecies fishery, but the catches of incidental species, known as by-catches. This situation can range from the small catch of a valuable species in a relatively clean directed fishery to a fishery directed at several more-or-less equally valuable resources (eg. a mixed groundfish fishery). Measurement and regulation of these by-catches is therefore an integral part of the management scheme of many fisheries. These by-catches are often expressed in terms of a ratio, either of the by-catch species to the main species, or of the by-catch species to the total catch in the fishery (Brown et al. MS 1973). Dramatic increases in these by-catch ratios within a fishery can lead to overruns of allocations of the by-catch species if they are not acted upon. Therein lies the key issue with regard to harvesting a multispecies resource which is subjected to a series of single species allocations: how to maximize the yield from all desired species without exceeding the allowable catch level of one or more of the species. The situation is complicated further when the availabilities of desired species differ by wide margins. For example, a vessel or fleet fishing for species A, which is very abundant, may be hampered in its efforts to achieve its allocation by the consistent by-catch of species B, which is not as abundant and for which it has either a small allocation or none at all. When this type of situation is spread over several fishery interactions, management of allocations becomes difficult, if not virtually impossible.

In many fisheries, eg. Grand Bank plaice and yellowtail (Brodie MS 1983), a standard practice to control by-catch has been to set a limit of a certain percentage (usually 10) of the by-catch species of the total catch for a trip of a vessel. These trip limits often vary during a year, depending on how fast the allocation of the by-catch species is being used. Other measures for regulation have included standard management practices such as closed seasons (Scotian shelf haddock), mesh regulations, and restricted areas, such as the small mesh gear box off Nova Scotia (Waldron MS 1979). However, each of these methods has obvious disadvantages. Imposition of trip limits promotes discarding of potentially valuable, but illegal, by-catches. Introduction of closed areas or seasons causes loss of yield from portions of stocks which would not normally be damaged by exploitation in that area or season. Also, under any management scheme, when the quota of one species is reached, further landings of this species can be reported as a different species (O'Boyle et al. MS 1983; Silvert and Dickie 1982). These are some of the difficulties which have arisen in attempts to regulate multispecies fisheries which are confounded by single species restrictions.

Analysis of Catch/By-Catch Data From Multispecies Fisheries

There have been several attempts at using general production curves (Schaefer 1954) to model multispecies fisheries over a wide geographic area. Horwood (1976) considered a hypothetical two species model and Pope (1976a) examined the cod and redfish stocks in NAFO Subarea 2-Div. 3K. Pinhorn (1976) used the Schaefer approach for the groundfish resource in NAFO Subareas 2 and 3 and Halliday and Doubleday (1976) used the same method for the groundfish fisheries on the Scotian Shelf (NAFO Div. 4VWX). These efforts considered only the total yield total effort concept and did not address any possible multispecies interactions. Pope (1976b) considered the effect of biological interactions between stocks in the form of yield models which dealt with competition and predator-prey relationships. In some of these studies (Pinhorn 1976; Halliday and Doubleday 1976) the authors compared the maximum sustainable yield calculations with the sums of the TACs of the individual species. In both these cases, the MSY was determined to be less than the sum of the species TAC's. Pope (1976b) stated that consideration of certain mixed fishery models did not support the adoption of total catch quotas without individual species limitations. He also felt that they were of some value in indicating that the MSY of a system may be less than the sum of the component MSY's and that they would be of more value in a situation where knowledge of individual stocks was somewhat incomplete.

Perhaps the most ambitious example of previous attempts at multispecies fishery management dealt with the multination, multispecies fishery which occurred in NAFO Subareas 5 and 6 in the 1960's and 70's. These fisheries encompassed over ten species, and saw involvement by more than twelve nations, with proposed allocations for 1974 totalling some 924,000 t (ICNAF 1974a). With the introduction of a national quota allocation scheme in 1972 (Brown et al. MS 1973) came the requirement that each country close a particular directed fishery when needed so that the sum of the main species catch and estimated by-catches in other fisheries would not exceed its allocation for that species. With such a large number of species-country interactions, control of quotas was extremely difficult. With this in mind, Brown et al. (MS 1973) devised a method to analyse the effect of by-catches on the realization of management objectives, based on national allocations and by-catch rates in the various fisheries.

Their analysis was based on linear programming (Glicksman 1963). Simply stated, the objective of their model was to maximize a value for the total catch of all species by all countries, with the constraint that no individual species allocation could be exceeded. This was determined by analyzing by-catch rates and subsequently calculating the optimum level of catch of the main species in each directed fishery. From the main species catches and the associated by-catches, the total catches for each species could be determined. The linear programming solution thus revealed optimum main species catch levels, based on previous years' by-catch data. However, the original solution was not practical, as it called for certain small but valuable main species catches to be reduced to zero. After additional constraints were added to the model, more realistic values for main species catches were obtained.

Once the ability to determine optimum total catch levels had been demonstrated for the Subarea 5 and 6 fishery, a concept known as two-tier quotas was introduced (ICNAF 1974b). This involved the setting of a total quota for all species combined which was somewhat less than the sum of the individual quotas for each species. The level at which this second tier quota was set was determined by linear programming and ideally should have represented the maximum catch level of all species, given that no individual species quota was exceeded. However, the second tier was often set above this figure, to allow for fluctuations in the by-catch patterns of the various national fisheries.

Since that time, there have been numerous other attempts at linear programming solutions to solve by-catch problems in multispecies fisheries. The procedure continued to be used for the Subarea 5 and 6 fisheries for several years (Brown et al. MS 1975, 1979), and Brodie (MS 1981) applied the same techniques to the Grand Bank groundfish fisheries. Murawski et al. (MS 1983) used a linear programming model to analyse the mixed species fishery system on Georges Bank, with the difference that fishing mortalities instead of catch quotas were used as constraints.

Although these linear programming exercises can provide solutions to otherwise unmanageable by-catch problems, these solutions are not always practical. In virtually all of the studies which employ this methodology, the authors point out the disadvantages of using historical by-catch data to determine future fishing patterns. Significant change in the abundance of one or more of the species in the system can cause considerable fluctuations in the by-catches in certain fisheries, resulting in the balance proposed by the linear programming model being upset. Brodie (MS 1981) also points out problems which can arise in interpreting by-catch ratios from certain main species fisheries where catches of two or more species tend to be constantly high. Also, the difficulties noted by Brown et al. (MS 1973) in proposing elimination of certain small directed fisheries often render certain solutions impractical.

Summary

In virtually all multispecies fisheries interactions which have been studied, a common feature has been the existence of individual species quotas. With this in mind, interpretation and analysis of these mixed fishery systems are often hampered by the lack of meaningful data. To speculate on what would happen in cases where management strategies were changed is extremely perilous, given the rapid changes known to occur in certain fisheries. Such are the problems in attempting to interpret multispecies fisheries interactions from within a single species framework.

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Discussion Period

Murawski: Sometimes the sensitivity of the models to the by-catch data can not be overemphasized. For example, in Subarea 5, with extended jurisdiction, the haddock stock was low for several years, and then a good year class came along which essentially recruited in one year. The quota had been set in the previous year on an assumed by-catch ratio, haddock could not be avoided, and fishermen could not maintain decent catch rates on other species while avoiding haddock. The entire system collapsed because it was predicated on a by-catch ratio from the previous year, with no account taken for changes in stock status.

O'Boyle: My experience is that very few fisheries are really mixed on a set by set basis; trip by trip perhaps, but set by set, fishermen are able to make very clean catches. I do not think that fishermen are quite as much random units moving around the ocean as we sometimes think.

Murawski: I would take issue with this but I think there is an element of stochasticity. For example, on a trip directed at cod, the fishermen may find good concentrations of yellowtail and decide to make it a yellowtail trip. There is that element of randomness.

Brodie: That affects interpretation of the data as well. The fishing effort for a day or even a trip is often classified as being directed at a species which initially was not the objective of the trip.

O'Boyle: This is a real issue for the managers since if the fish are avoidable, there will be a different strategy than if they are unavoidable. What can be determined by examining observer data?

A. Sinclair: There is a lot of effort spent fishing in areas that yield pure catches, but it depends largely on the circumstances of the trip. Large offshore vessels which are company-directed are given a list of species to catch. Obviously, the best way to obtain these is to fish where the catches are pure. Small owner-operated vessels can fish in areas where catches are highly mixed. The patterns are quite variable and can affect the linear programming models.

Brodie: There is also the problem that for these models, by-catch ratios are summed over long periods of time, often an entire year.

Murawski: I think the linear programming model has several limitations since it is a retrospective analysis, but it is an interesting format to utilize some of the questions of fishery economics as constraints in a bioeconomic model. Implicit in the formulation is the idea that the biological analysis can be weighted by some kind of economic slippage in the system. If emphasis is to be placed on one fishery over another, a value constraint can be placed on a particular fishery. This was evident in Brown's analysis which called for elimination of most of the US domestic fisheries in favour of international fisheries because they were relatively clean. The formulation will always favor the cleanest fisheries, but there are many sociopolitical ramifications of a pure solution like that. I think the linear programming analysis is very amenable to adjusting the economic factors so it should not be abandoned just because it is sensitive to by-catch levels.

Brodie: Often, more complex constraints are required for the linear programming models. The situation is usually more involved than can be determined from examining catch levels and by-catch ratios.

Analysis of By-Catches Observed in the Scotian Shelf Foreign Fishery and Their Impact on Domestic Fisheries

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Waldron, D.E. and A.F. Sinclair. 1985. Analysis of By-Catches Observed in the Scotian Shelf Foreign Fishery and Their Impact on Domestic Fisheries. p. 60-91. In: R. Mahon [ed.] Towards the Inclusion of Fisheries Interactions in Management Advice. Can. Tech. Rep. Fish. Aquat. Sci. No. 1347.

Abstract

A study to investigate the Scotian Shelf small mesh management policy was conducted from 1977-1982. From 1977 to 1979 vessels of Cuban and Soviet registration were permitted to fish on the shelf. This provided an unique opportunity to not only analyse the placement of the small mesh gear line but also to model the fishery's impact on recruitment to other domestic large meshed fisheries.

Geographic distributions of by-catch for cod, haddock, and pollock support the placement of the small mesh gear line (SMGL). In general larger by-catches of all species were observed in areas landward of the SMGL. Calculation of yields to the domestic fisheries of cod and haddock which were caught by the foreign fleets provided insight into past fisheries. Of the total tonnage caught only 50% would have been returned to the domestic fishery. This was a result of availability and the fact that natural mortality of these small fish is much greater than the fishing mortality which the domestic fleet could exert.

A model was used to evaluate future gains to the domestic cod and haddock fleets if the foreign fishery ceased. It showed very little benefit. Increasing the mesh size of the foreign fleets from 60 to 90 mm resulted in a 1% increase in the TAC for haddock and no increase in cod TAC (in fact a loss occurs). However, the impact on the silver hake stock and catch size is dramatic. If the current level of fishing effort is maintained the loss in catch is estimated to be between 30-40%. In order to fish at $F_{0.1}$ the foreign fleet must increase its effort by 30%. Within the context of this paper such an increase is considered an unnecessary expenditure of fishing power. There also appears to be little benefit to the silver hake stock. Further, there is minimal reduction in the cod and pollock by-catches. While haddock by-catches could be significantly reduced. The by-catch of this species is in the order to 100-500 tons/year.

In conclusion it would seem that the current management of the Scotian Shelf small meshed fishery is adequate to minimize by-catch and permit access to the silver hake stock. Historical haddock and cod losses to the domestic fleet catch is minimal. Future increases in domestic yield of these species is minimal.

Introduction

The presence of a foreign fishery using small mesh gear for silver hake on the Scotian Shelf since the early 1960's has led to concern that there may be adverse effects on recruitment to the cod and haddock stocks. Indeed, there was supporting evidence, with large haddock catches reported from this fishery in 1964 and 1965. Recruitment and eventually population size declined rapidly afterwards (Halliday 1971; Waldron 1980). Similarily, for cod it was demonstrated that by-catches of young cod (ages 1-2) of about 4,000 t annually could account for the reduced recruitment observed in the early 1970's (Anon. 1976a). Since then, the decline in the cod stock biomass has been attributed to growth overfishing by foreign fleets (Anon. 1977a; Maguire et al. 1982).

The Standing Committee on Research and Statistics of the International Commission for the Northwest Atlantic Fisheries (ICNAF) reviewed the distribution of the silver hake fishery in relation to other groundfish at the Ninth Special Commission Meeting in December 1976 (Anon. 1977b). Three areas were identified as being actively fished for silver hake. In one area along the edge of the Scotian Shelf, Canadian research survey results indicated the least overlap of distributions of other important commercial species with silver hake. The committee noted however that:

> "... the northern limit of this fishery area is critically important, as the haddock could be subjected to by-catch problems, particularly in the winter when they are aggregated in prespawning and spawning concentrations. These aggregations can occur to a depth of 155 m (85 fath.) in winter (November to March or April inclusive), depending on hydrological conditions. However, in summer (May to October inclusive), haddock occur in shallower areas, and fishing for silver hake along the edge of the continental shelf in depths as shallow as 120 m (65 fath.) would avoid the main areas of haddock distribution".
>
> (Anon. 1977b)

Based on this advice, ICNAF agreed to a regulatory proposal allowing fishing with small meshed gear (60 mm) south and east of a line defined along the edge of the Scotian Shelf between April 15 and November 15 (Anon. 1976b). The line has become known as the Small Mesh Gear Line (SMGL) (Figure 1). The specific proposal resulted from negotiations between Canada, the originating nation, and the USSR and Cuba as the nations primarily involved in the silver hake fishery.

When Canada extended jurisdiction to 200 miles offshore in 1977 the ICNAF regulation was accepted and codified in the Canadian Foreign Vessel Fishing Regulations. In addition regulations were introduced to limit by-catch of haddock to less than 1% and that of other important commercial species to less than 10% of the total weight onboard the vessel. Enforcement of these regulations in part requires Canadian fisheries observers trained in both the fishing regulations and in techniques of biological sampling. These observers are deployed in the foreign fleets to collect information vital to monitoring and managing the small meshed fisheries (Kulka and Waldron 1983).

In the years immediately following 1977 the biomass of cod and haddock populations rose rapidly providing the base for a new expansion of the Canadian inshore and offshore groundfish fisheries. To what extent the restriction of the small mesh fishery contributed to this "boom" may never be known. Important management lessons are to be learned, however, from exploring these issues. It is therefore not surprising that questions concerning the current small meshed fishery are being asked. These include concerns over the current by-catch levels and their direct effects on Canadian fisheries. Do the current by-catch, mesh size, and fishing area/season regulations effectively reduce recruitment overfishing of cod and haddock stocks? Would the advancement of the silver hake fishing season permit the fleets to catch their allocations sooner, avoiding by-catches of cod and haddock later in the season? Are there areas landward of the SMGL where acceptable silver hake catch rates with minimal by-catch could be obtained by the foreign fleets? Further, would it be more advantageous to employ the current by-catch regulations on an individual vessel or fleet basis.

This paper addresses the above issues using data from recent stock assessments and that collected by Canadian fisheries observers deployed on foreign commercial fishing vessels on the Scotian Shelf. It attempts to evaluate the current management system in relation to its benefits to all nations fishing on the Scotian Shelf.

Methods

Monthly nominal catch estimates for silver hake, squid, cod, haddock, and pollock by Cuba, USSR, and Japan for the years 1977-1982 were obtained from Northwest Atlantic Fisheries Organization (NAFO). Catches in 1983 were obtained from the Federal Licensing and Canadian Surveillance Hierarchical database (FLASH). Monthly by-catch rates for cod, haddock, and pollock were calculated from data collected through the Canadian International Observer Program (IOP) (Kulka and Waldron, 1983). By-catch rates were computed as percent of total observed catch and no distinction was made among directed fisheries. These by-catch rates were then applied to reported total finfish and squid nominal catches of Cuba, USSR, and Spain to estimate total catches of cod, haddock, and pollock. IOP data used in this analysis were available for the periods 1977-1979, 1982-1983. Data from the latter period were extensively edited and considered final. Data from the earlier period were not as extensively edited and may change with further editing.

Cuba and the USSR took the majority of the total silver hake catch on the Scotian Shelf and by considering only their catch a general picture of the monthly distribution of the silver hake fishery was obtained. Silver hake catch by these two countries was aggregated by 10' square and month over the years aforementioned and plotted.

Shading of the boxes in these plots is in proportion to the maximum aggregated value in each plot (i.e. aggregated catch per box/largest aggregated catch for the plot). Squares with aggregated values less than 1% of the maximum value were not included in the plots.

Squid catch by Japan, Cuba, and the USSR were aggregated and plotted in a similar manner. These three countries consistently took the majority of the total squid catch.

By-catch rates of cod, haddock, and pollock in the Cuban and USSR fisheries were calculated from catch data aggregated by units (10' square) and month over all years. Only those squares where the total catch was greater than or equal to 1% of the maximum aggregated monthly total catch were plotted. The boxes were shaded to indicate critical by-catch levels.

The geographic distribution of mean age of haddock by-catch in the small mesh fisheries was determined using modal lengths. IOP length frequencies for 1982 and 1983 from Japanese, Cuban, and Soviet vessels were used. Using age length keys, three prominent length groups were found corresponding to age 1, 2, and 3+ fish. The thresholds used to determine age group were less than 25 cm for age 1, between 25-34 cm for age 2 and greater than 34 cm for age 3+. Individual length frequencies were summed by month and 30' of longitude along the shelf edge. The mean age group in each case was determined and plotted. A similar analysis was not possible for cod and pollock because the fish were too large to allow modal analysis. Ageing of these samples was not extensive enough to allow the breakdown of age groups on so fine a geographic scale.

Removals-at-age for 4VsW cod (1978-1982) and 4VW haddock (1977-1982) are from Gagné et al., 1983 and for haddock, Mahon et al., 1983. Fishing

mortalities-at-age presented in Gagné et al., 1983 and Mahon et al., 1983 were perturbed to reflect the domestic fishery using the method presented in O'Boyle et al., 1981. In the context of this paper two fleet components are considered, the foreign fishery (1) of OTB's using bottom trawls with 60 mm codends and the otter trawl domestic fishery using 130 mm codends (2).

Total F can be subdivided into;

$$F_1 = \frac{C_1 \times F_{TOT}}{C_{TOT}} \qquad (1)$$

for fleet component 1. Similarly for fleet component 2;

$$F2 = \frac{F_{TOT} \times (C_{TOT}-C_1)}{C_{TOT}} \qquad (2)$$

where C_{TOT} = total catch-at-age
F_{TOT} = total F-at-age
C_1 = catch-at-age for the foreign fishery
F_1 = fishing mortality-at-age for the foreign fishery
F_2 = fishing mortality-at-age for the domestic fishery

Using this new F value (F_2) in Baranov's catch equation (Ricker, 1975) provides a method to calculate the expected catch by the Canadian fishery of fish caught by foreign vessels during the 1977-1982 period. The method assumes no changes in availability or increases in observed effort (F). The mechanics of this are as follows:

Calculate the numbers of fish which are present at the start of the next year:

$$N_t = N_0 e^{-Z} \qquad (3)$$

where Z = (new fishing mortality calculated above) + M (where M = natural mortality = 0.2). These fish will be referred to as the survivors (S).

Calculate the numbers of fish which are caught during the year;

$$C = \frac{FAN}{Z} = \frac{F \times (1-e^{-Z}) \times N_0}{Z} \qquad (4)$$

The survivors of year t are added to the foreign catch in year t+1 to give the new numbers available for fishing in year t+1.

$$N_{1(t+1)} = S_2 + N_{t+1} \qquad (5)$$

These new numbers ($N_{1(t+1)}$) are fished in year (t+1) and calculated in equation (3) where $N_0 = N_{1(t+1)}$. The survivors in N_{t+1} are added to the foreign removals, equation (5) and the process is repeated for each cohort until all of the fish caught prior to 1982 have gone through the fishery.

Would removing the foreign small meshed fishery from the Scotian Shelf have an impact on the haddock and cod stocks? This scenario was studied using equation (2) above with removals-at-age and F matrices for both cod (Gagné et al., 1983) and haddock (Mahon et al., 1983). Partial recruitment for only the domestic fleets were calculated, from equation (2), as the quotient of the F's at age divided by the F at full recruitment. A flat top partial recruitment curve was assumed after full recruitment, which was age 6 for both species.

Mean weights-at-age, population and catch numbers-at-age for 1982 as well as the recommended level of recruitment were taken from the above quoted papers for both cod and haddock. These were used as input parameters in the calculation of a new Y/R using the model of Thompson and Bell (Ricker, 1975). Projections at the recommended 1983 $F_{0.1}$ and the new $F_{0.1}$ levels for both cod and haddock (Anon. 1983) were made using the MPROJECT function of Rivard (1982).

Change in haddock and cod by-catch in the foreign fishery if the codend mesh size increased from 60 to 90 mm were calculated as follows.

O'Boyle et al. (1981) modified Pope et al.'s (1975) logistic equation for selectivity at length to be:

$$S(L) = \frac{1}{1 + \exp[\alpha + \beta (L/L50)]} \qquad (6)$$

where S(L) = fraction of fish selected at length (L)
α = intercept
β = slope
L = length of fish
L_{50} = 50% retention length (in units of L)

Equation 6 can be linearized as:

$$\ln \frac{1-S(L)}{S(L)} = \alpha + \beta (L/L_{50}) \qquad (7)$$

The 50% retention lengths were determined by the equations of Clay (1979a):

L_{50} = 3.63 m - 28.49 for haddock
L_{50} = 4.35 m - 87.62 for cod

where m = codend mesh size in mm. Values of m = 60 and 90 were used in this study for haddock and cod.

Equation 7 was solved using selection ogives for cod and haddock at mesh sizes of 76 and 100 mm from Hodder (1964). No ogives at 60 and 90 mm were available. It was assumed that the ratio of selectivity at 76 to that at 100 would be similar to the ratio of selectivity at 60 to that of 90 mm.

NAFO investigated the management option of increasing the mesh used in the silver hake fishery from 60 to 90 mm (Anon. 1980, 1981). This paper further investigates this scenario. Selectivity at length for 60 and 90 mm codends were calculated using the slope to intercept values from equation 7 substituted in equation 6.

Observed length frequencies from the 1977 to 1983 (excluding 1980) foreign small mesh fishery were perturbed using the ratio of selection at length for 60 mm divided by selection at length for 90 mm codends. Both the observed and newly perturbed length frequencies were converted to weight at length using weight/length relationships from the foreign fishery for each year modeled. These weights at length were summed to give an observed and perturbed weight. The ratio of perturbed to observed weight gives the reduction in current by-catch levels for cod and haddock which could be expected if the codend mesh size was increased from 60 to 90 mm.

The impact on silver hake yields from a shift in codend mesh size from 60 to 90 mm was also studied. A new partial recruitment pattern for silver hake was calculated as below using data for 1982 from Waldron et al., 1983.

$$PR_{90} = PR_{60} \times Sel_{90}/Sel_{60}$$

where PR_{90} = Silver hake partial recruitment at age if the fishery uses 90 mm codends.

PR_{60} = Silver hake partial recruitment at age calculated for the current fishery which uses 60 mm codends (Waldron et al., 1983).

Sel_{60} = The selectivity at age for silver hake when 60 mm codends are used in the fishery (Clay, 1979a).

Sel_{90} = The selectivity at age for silver hake if 90 mm codends were used in the fishery (Clay, 1979a). Sel_{90} is calculated using the partial recruitment pattern when 40 mm codends were used.

A Thompson and Bell yield per recruit model was employed to calculate a new $F_{0.1}$ for a 90 mm fishery. Catch and population numbers in 1982, average recruitment, mean weights, and $F_{0.1}$ at 60 mm utilized in the Rivard (1982) MPROJECT program were from Waldron et al. (1983).

Results

Silver Hake

Monthly catches of silver hake by Cuba and the USSR combined are given in Table 1. Since 1977 these catches have ranged from 35,000 t to 59,000 t. Peak catches are taken in the May to July period. Since 1977, these vessels have been restricted to fishing in the period April 15 - November 15. The first two weeks of the fishing period have been used to search for favorable concentrations of silver hake. Catch then increased through the months of May, June, and early July. By mid-July silver hake became less abundant in the area seaward of the SMGL (Figure 1) and subsequently the overall catch of the species declined.

The geographic distribution of observed silver hake catches by month is given in Figure 2. These plots include observation from the 1977-79 period when selected vessels were permitted to fish landward of the SMGL. The monthly trend in catches indicated that in April and May silver hake were found predominantly along the edge of the shelf. Good catches of hake were taken landward of the SMGL in June and July but most of the catch was still taken along the edge. From September to November most of the observed silver hake catch was taken on the shelf. This pattern closely resembles the movement of the Soviet fleet in the 1960s (Clay 1979b). Since 1980 fishing landward of the SMGL has been prohibited.

Catches of silver hake by Japan are given in Table 2. For the period 1977-81 these catches have been by-catch in the Japanese squid and argentine fisheries. In 1982 and 1983 Japan had a national allocation for silver hake. Overall silver hake catch by Japan has been minimal in comparison to that of Cuban and the USSR.

Squid

Monthly squid catches by Cuba and the USSR are given in Table 3. The highest yearly catch of squid by the two countries was in 1977 and since then the catches have declined to very low levels. Since 1978 squid catches peaked late in the season following the main silver hake fishery. Often the same vessels remained in the area for both fisheries and redirected their fishing to the increasingly abundant squid as silver hake abundance decreased.

Monthly squid catches by Japan are given in Table 4. The peak catch of squid by Japan was in 1979 and catches have decreased drastically since then. On a monthly basis peak catches were usually taken in August or September with the exception of 1978 when the highest catch was in October.

The monthly observed catch distribution of squid by Japan, Cuba, and USSR on the Scotian Shelf are given in Figure 3. Through the period April-June squid catches were much less than catches of silver hake with the highest catches being taken in association with high silver hake catches. In July, squid and silver hake were taken in comparable quantities and in similar areas seaward of the SMGL. Landward of the SMGL squid catches were small. For the period August to November squid catches dominated silver hake seaward of the SMGL and in the extended area (see Figure 1). In September-November the squid fishery was highly concentrated in the extended area.

By-catch

To estimate total by-catch levels for cod, haddock and pollock, by-catch rates calculated from IOP data were applied to total finfish nominal catches reported to NAFO. Monthly total finfish nominal catches for Cuba and USSR combined, and Japan are given in Tables 5 and 6 respectively.

Cod

Monthly reported catches of cod by Cuba and

the USSR combined are given in Table 7. The highest reported catches come from the months of June through August. Yearly total reported catches ranged from 78 t in 1982 to 697 t in 1979. Most of these catches were reported from Division 4W and Subdivision 4Vs.

Monthly cod by-catch rates for Cuba and the USSR calculated from IOP data are given in Table 8. On a monthly basis the rates were never above 2% and were usually less than 1% of the total catch. Using the by-catch rates given in Table 8 monthly catches of cod were estimated to check the accuracy of the reported quantities. These estimates are given in Table 9. The largest differences between reported and estimated catches were in 1979 and 1981 when the reported catches were 274 and 231 t greater than the estimated catch respectively.

Monthly reported catches of cod by Japan are given in Table 10. The highest yearly reported catch was 14 t in 1980 and there was no apparent trend in monthly catches across years. Monthly by-catch rates calculated from IOP data are given in Table 11. None of the individual monthly values were above 2% and only 4 were above 1%. These by-catch rates and total finfish nominal catches (Table 6) were used to estimate monthly by-catch levels. The results are shown in Table 12. While most of the yearly totals for estimated and reported catch were in close agreement, a large discrepancy was found in 1978, notably in November. The estimated cod catch was 82 t while only 3 t were reported to NAFO. The total estimated catch for 1978 was 8 times that reported to NAFO.

The monthly geographic distributions of observed cod by-catch by Cuba and the USSR is given in Figure 4. In the months April-July there were very few units where the by-catch level of cod was above 1%. However, in August several squares had by-catch in the 3-10% range and in an unit by-catch was above 10%. By-catch was highest along the shelf edge and an area east of Emerald Basin landward of the SMGL. In the months of September-November the observed by-catch levels were below 1% in most units.

Haddock

Monthly reported catches of haddock by Cuba and the USSR combined are given in Table 13. Yearly total reported catches ranged from 34 t in 1977 to 292 t in 1983. Most of these catches were reported from Division 4W and Subdivision 4Vs.

Monthly haddock by-catch rates for Cuba and the USSR were calculated from IOP data and are given in Table 14. The highest by-catch rates, in July of 1983 (33.28%) and September of 1982 (10.67%) were observed in months of comparatively little fishing activity by these countries (Table 5) at a time when the year's fishery was coming to an end. Including these two months there were eight months when the observed by-catch rate of Cuba and the USSR was above the 1% regulated level. There was a tendency for haddock by-catch to be highest in the final month of the silver hake fishery.

Monthly haddock catches estimated using the observed by-catch levels (Table 14) and monthly reported finfish plus squid landings of Cuba and the USSR (Table 5) are shown in Table 15. The estimated total catches were greater than the reported catches in all years. The greatest difference was in 1977 when the reported haddock catch was 34 t and the estimated catch was 837 t. Since then, the largest difference was in 1983 when the estimated catch exceeded the reported catch by 289 t.

Monthly reported catches of haddock by Japan are given in Table 16. The highest yearly reported catch was 50 t in 1978 followed by 47 t in 1983. Monthly by-catch rates calculated from IOP data are given in Table 17. By-catch rates in 1983, 1982, and October of 1981 were all above the 1% regulated level. The highest monthly by-catch level was 4.53% in August of 1983. These high by-catch levels occurred when there was comparatively little fishing activity by Japan (Table 6) and when most of the fishing was of an exploratory nature, looking for squid.

Monthly estimates of haddock catches by Japan obtained using observed by-catch rates (Table 17) and reported total catches (Table 6) are given in Table 18. The highest yearly estimated catch was 55 t in 1983. The largest discrepancies between reported and estimated catches occurred in 1977 and 1981 when the reported catches were 23 t and 18 t less than the estimated catches respectively.

The geographic distribution of haddock by-catch by Cuba and the USSR is given on a monthly basis in Figure 5. In April by-catch has been very low with only one unit showing a rate in excess of 1%. In May by-catch in excess of 1% was experienced in the east-central portion of the shelf edge and in the area of Emerald Basin. In June- August haddock by-catch between 1-10% was experienced in many areas along the shelf edge. The highest levels of by-catch were found around Emerald Basin and to the east of the Emerald Basin landward of the SMGL. In September-November by-catch in all areas decreased but there remained areas where the rate was in excess of 1%.

Pollock

Monthly reported catches of pollock by Cuba and the USSR combined in 1977-1982 are given in Table 19. Most of the pollock catch came from the months of May and June. The highest yearly reported catch was 1072 t in 1979 and the lowest was 147 t in 1977.

Monthly pollock by-catch rates for Cuba and the USSR calculated from IOP data are given in Table 20. Generally pollock by-catch rates were observed to be highest in April-June, the early portion of the silver hake fishery, and lowest in the later months. While monthly by-catch rates were above 1% in 14 instances there were none greater than 10%.

Monthly pollock catches were estimated using the observed by-catch rates (Table 20) and monthly reported finfish plus squid landings of Cuba and

the USSR (Table 5). The results appear in Table 21. There was close agreement between the reported and estimated yearly total catches for pollock. The largest difference between reported and estimated catches occurred in 1983 when the estimated catch exceeded the reported catch by 232 t.

Monthly reported pollock catches by Japan are given in Table 22. The highest yearly catch was 107 t in 1978 followed by 81 t in 1980. Monthly by-catch rates indicated very low by-catch levels in this fishery (Table 23) with an increase in recent years. All monthly values were less than 1%.

Monthly estimates of pollock catches by Japan obtained using Tables 6 and 23 are given in Table 24. The highest annual estimated catch was 68 t in 1978. The largest yearly difference between estimated and reported catch was in 1978 when the reported catch exceeded the estimated catch by 39 t.

The geographic distribution of pollock by-catch by Cuba and the USSR is given on a monthly basis in Figure 6. In April, by-catches above 1% were observed in only three geographic units. In May and June there were higher by-catches on the shelf edge south of Emerald Bank and in the vicinity of Emerald Basin landward of the SMGL. In July-September by-catch was reduced in these regions but was higher in the eastern portion of the SMGL around the "Gully".

Mean Age of Haddock By-catch

The age distribution of haddock by-catch along the shelf edge was examined using sampling data from Japanese, Cuban, and USSR observer trips in 1982 and 1983. Modal analysis was used to separate 3 age groups, age 1, age 2, and age 3+. The mean age group by 30' of longitude was determined by month and plotted in Figure 7. The general trend demonstrated was that the mean age of haddock at either end of the SMGL was higher than in the middle region. Specifically, in the region of 61°-62°W longitude, south of Western and Sable Island Banks, the haddock tended to be younger then around 63°-64°W longitude and 60°W longitude.

Potential Domestic Yields from Foreign Removal of Cod and Haddock

Foreign removals of cod and haddock have been predominantly from the 4VsW and 4VW stocks respectively. Catch-at-age estimates of 4VsW cod by the USSR (1978-1982) and of 4VW haddock by all foreign countries (1977-1982) are given in Tables 25 and 26 respectively. For cod the removals were mainly composed of ages 3-5. For haddock in 1977-1979 ages 2-4 dominated and for 1980-1982 the removals were mainly at age 1.

Total fishing mortalities for 4VsW cod (Gagné et al. 1983) and 4VW haddock (Mahon et al. 1983) were perturbed to represent domestic fleet mortality only by the procedure outlined above. The resulting partial mortality tables are shown in Tables 27 and 28 for cod and haddock respectively. Potential domestic fleet catches of the foreign removal were calculated with the method outlined in equations 4 and 5.

From 1978-1982 the estimated USSR catch of 4VsW cod was 2,173 t (Table 29). The estimated potential domestic catch of these removals were 1,314 t for the same time period or 60% of the USSR catch. However, after 1982 the survivors would continue to provide yield to the domestic fishery. Assuming that for 1983 onward the domestic fleet fished at $F_{0.1} = .2$, the survivors would yield an additional 1,266 t over the next 13 years. In total, it was estimated that the USSR by-catch would yield 2,580 t to the domestic fishery over 19 years. This is 1.2 times the estimated foreign by-catch for 1978- 1982.

For 4VW haddock the estimated foreign catch from 1977-1982 was 1,733 t (Table 30). Over the same period, the estimated domestic catch of these removals was 1,006 t or 58% of the foreign catch. After 1982, the estimated yield of the survivors to the domestic fleet fishing at $F_{0.1} = .22$ was 2,323 t. In total, the estimated domestic catch of the foreign removals was 3,329 t over 19 years. This was 1.9 times the estimmted foreign by-catch.

The results of the Y/R analysis used to describe a change in yield if the foreign fishery was removed are presented in Tables 31 and 32 for haddock and cod consecutively. There is an average gain of 145 t/year for haddock and an average loss of 20 t/year for cod. The loss in cod catch is based on the very close similarity of the partial recruitment patterns for fish caught using 60 and 130 mm codends by the foreign and domestic fleets respectively. In fact very little of the foreign catch of cod would be transferred to the domestic fleet if such an increase in mesh size were to evolve. Overall in the long term projections, for haddock there is a net increase of roughly 1% and a loss of less than 1% for cod.

Yield Changes for the 4VWX Silver Hake Fishery

If the management measure of increasing the silver hake fishery codend mesh sizes from 60 mm to 90 mm is implemented the probable impacts on the silver hake fishery must be considered. Changes in the partial recruitment pattern from 60 to 90 mm codends causes an increase in fishing mortality at $F_{0.1}$ of 36% (Table 3). This change in fishing mortality from .418 to .568 results in an overall loss of 6162 t in catch over 8 years (Table 34, Figure 8). Since recruitment is held constant at 1.46 billion fish there is little difference between the estimated size of the population under fishing pressures at the two $F_{0.1}$ levels (Table 35, Figure 9).

Considering the unlikely scenario that the fleet would not increase its effort to a new $F_{0.1}$ level but remain at the same level indicates the loss is more severe. This loss in catch is estimated at 98,000 tons after 8 years on a yearly average of 12,300 tons per year. The losses of course are much larger in the first few years (Figure 8, Table 34).

The population biomass is as expected under the two new levels of $F_{0.1}$ (Figure 9). The different size of the population increases dramatically when the $F_{0.1}$ at 60 and 90 mm was compared to fishing mortality at 90 mm equal to that at $F_{0.1}$ for 60 mm gear. After 8 years, the net gain in this case is some 273,000 t or 34,200 t per year (Table 35).

By-catch Reduction for the 4VWX Silver Hake Fishery

Estimated reductions in by-catch of cod, haddock, and pollock if the small meshed silver hake fishery used 90 mm rather than 60 mm codends are presented in Figure 10. Changes in selectivity are most evident for cod and haddock but not pollock. Shape of these species is no doubt influencing the selectivity pattern. Cod less than 42 cm (or less than age 2, 3) are only partially selected with full selection of fish greater than 42 cm in length. Haddock are partially selected until 64 cm but in reality they are fully selected after a length of 52 cm (age 4-5). Pollock are partially selected to a length of 77 cm. Like haddock, they are really partially selected to a length of 53 cm (age 4-5).

Discussion

Since the extension of jurisdiction in 1977 segments of the Canadian fishing industry have continued to express the concern that the small mesh fishery on the Scotian Shelf still catches considerable quantities of cod, haddock and pollock. We have addressed this concern by collating the official reported statistics, examining by-catch rates from the IOP, and estimating by-catches for comparison with the reported data. The underlying assumptions has been that the observed monthly by-catch rates were indicative of the total fleet by-catch rates and that the total finfish plus squid nominal catches reported to NAFO are accurate. To the best of our knowledge these assumptions cannot be rejected.

Since 1977 the small mesh fishery catch of cod, haddock, and pollock has been minimal in comparison to the total stock catches. For 4VsW cod the reported catch in the small mesh fisheries was 5% of the total in 1977 and this decreased to less than 1% (Gagné et al. 1984) (in 1983 1,218 t were taken by Portugal under a non-surplus national allocation using 130 mm gear). For 4VW haddock the reported small mesh catch ranged from 1-5% of the total (Mahon et al., 1983) while for 4VWX+5 pollock the small meshed catch never exceeded 2% of the total. For these 3 species the differences between reported and estimated catches for Cuba and the USSR combined were never greater than 300 t for any 1 year. In 1983 the estimated haddock catch was 289 t greater than the reported catch (Tables 13 and 15). In 1979 the estimated cod catch was 274 t less than the reported catch (Tables 7 and 9). Given the low level of reported catch and the small differences between reported and estimated catches relative to the total catch one may conclude that the current management regime has been successful in limiting the catch of cod, haddock and pollock in the small mesh fisheries.

The plots of by-catch levels of cod and haddock (Figures 4 and 5) indicated higher by-catches landward of the SMGL. This supports the conclusions of ICNAF (Anon. 1977b) based on the results of Canadian summer research surveys. Recent work by Scott (1982) has indicated large concentrations of 0 and 1 group haddock on the banks just north of the SMGL. Thus it would be likely that a northward movement of the SMGL could result in increased by-catches of cod and haddock.

Examination of the mean age of haddock by-catch in the 1982 and 1983 small meshed fisheries revealed that younger fish were caught in the eastern portion of the area fished from approximately 62° to 62° longitude. It is the catch of young haddock which has the greatest potential impact on Canadian fisheries. At the current levels of by-catch these impacts has been minimal. However any increases in by-catch of young fish will only increase these potential impacts.

The question of how much of the fish caught by the foreign fleets could have been caught by the domestic fleets was addressed using an age-structured model. Using this approach it was assumed that domestic effort and thus fishing mortalities would have remained the same in the absence of foreign effort, that the fish caught in the foreign fishery would have been equally available to the domestic fishery, and that the partial fishing mortalities used were indicative of the domestic fleet. The assessments for 4VsW cod (Gagné et al. 1983) and 4VW haddock (Mahon et al. 1983) indicate that perhaps the more recent F's were higher than those used here. However, this would have a minimal effect on the results.

The projected domestic catches of foreign removals indicated that in the period 1977-1982 the domestic fleet would have only caught a fraction of the weight caught by the foreign fleets, 60% for 4VsW cod and 58% for 4VW haddock. For 4VsW cod the Canadian catch was 221,410 t from 1978-1982 and the projected additional catch for the same time period from the foreign removals was 1,314 t, an addition of .6%. For 4VW haddock the Canadian catch was 60,977 t from 1977-1982 and the projected additional catch was 1,006 t or 1.7%. The survivors in 1982 would continue to yield catch to the domestic fishery into the 1990's. In the long term the USSR removals of cod were estimated to yield 2,580 t to the domestic fishery over a 19 year period. This was 1.2 times the estimated foreign catch for 1978-1982. The long term estimated yield of the foreign catch of haddock was 2,323 t or 1.9 times the 1977-1982 foreign catch. The reason for this higher estimated haddock yield is that the foreign catch was mainly of age 1-2 fish thus making the potential impact greater than for cod where the USSR removals were mainly age 3-5.

The argument that removing the foreign fishery would increase future yields is doubtful. This scenario was investigated using an age

structured model which employed different selectivity patterns. The increase in $F_{0.1}$ for haddock of 2% is offset by an increase in the yield per recruit of 2% with a yield per unit of effort increase of 4%. The overall gain to the fishery is small, only 1%, for an increase in effort of 2%.

These effects, as well as those of the previous model, are so slight that the only conclusion one can draw is that there is little effect on the haddock or cod fishery whether the foreign vessels fish or not.

Clay (1979c) and Clay and Halliday (1980) suggested that the Scotian Shelf silver hake fishery could move from 60 mm to 90 mm codends with a subsequent small increase in the yield per recruit and average weight of fish. Clay (1979c) further points out that there would be a 10 to 30 percent increase in effort. The Thompson and Bell yield per recruit models used in our study agree with those observations and indicated that in order to maintain an $F_{0.1}$ fishing level, effort would need to be increased by 36%. The yield per recruit would not increase and the average weight would increase by only 1%.

Modeling the silver hake population at different levels of $F_{0.1}$ and mesh size results in the observation that both catch and population biomass at $F_{0.1}$ for 60 and 90 mm codends are similar (Figures 8 and 9). Within the models ability to predict and the fact that constant recruitment is assumed these estimates can be considered equal. No increase in effort with an increase in mesh size results in the population stabilizing at a level 10% higher than that estimated for a 60 mm fishery and the catch stabilizes at a level 12% below that estimated for 60 mm.

The constant effort case is an inefficient use of the silver hake resource. Although effort is not increased the loss in catch is very large in the first three years. As more fish escape the 90 mm trawl gear the population gains in strength relative to that estimated for a fishery using 60 mm gear. The impact of the increase in population is not considered within the context of this paper.

There seems little or no benefit to the silver hake fishery of an increase in codend mesh sizes. The only benefit may be in a reduction in by-catch of species such as cod, haddock, and pollock.

By-catch reductions are based upon the selectivity of 60 and 90 mm gears. Both cod and pollock by-catches would be reduced by less than 10% on the average. Haddock by-catches would be substantially reduced. This is the result of the small meshed fishery overlapping juvenile haddock rearing areas. Although, to some degree, there maybe a relationship between recruitment and reduction in by-catch, there could be some link to fluctuations in distribution of juvenile fish. This is no doubt due to an environmental parameter.

If the catch of silver hake is to be maintained at the current level after a switch to 90 mm gear effort would have to increase. This would mean greater exposure of cod and haddock to the gear and possibly cancel any reduction in by-catch due to changed selectivity.

Waldron and Gray (1978) conducted an experiment to investigate the appropriate combination of area and gear which would permit a viable squid fishery with minimal by-catches of cod and haddock. Four areas were selected: Emerald Bank; Sable Island Bank seaward of the SMGL; Sable Island Bank adjacent to Sable Island; and Banquereau Bank. Three gear types, otter trawl, off bottom bobbin, and off bottom chain were compared. In an area (area 3) to the landward side of the SMGL the most cod and haddock were caught. Otter trawls in general caught more of these two species with the off bottom chain gear catching least. Area 3 had the largest catches of cod and haddock while area 1 had the least. There was in particular a significant interaction between gear and area fished. Otter trawls fishing in area 3 (landward of the SMGL) caught more cod and haddock than any other gear - area combination.

All gears caught more cod and haddock in areas landward and eastward of the SMGL (areas 2 and 3) when compared to fishing areas inside the SMGL. Such results supported the initial ICNAF reasons for establishing the SMGL as an area which would reduce by-catches of domestically desirable commercial species.

Interestingly, the area with the highest catch rates of silver hake was area 2, eastward of the SMGL. Further, otter and off bottom bobbin trawls were very similar in silver hake catch rates while off bottom chain trawls had a low catch rate for silver hake. Although the use of off bottom gear could reduce by-catches of cod and haddock they would also reduce substantially the catches of the target species silver hake.

It would seem most unreasonable to enforce a regulation favouring a change in gear used, from otter trawls to off bottom trawls, for the silver hake fishery. The analysis of Waldron and Gray (1978) further suggests that areas seaward of the SMGL, although perhaps not the best, have catch rates which are certainly adequate for the silver hake fishery.

Seasonal distribution patterns for the squid and silver hake foreign fisheries support the contention that areas seaward of the SMGL have the best catches of both squid and silver hake (Figures 2 and 3). In agreement with Waldron and Gray (1978) the highest catches of cod and haddock occur in areas landward of the SMGL (Figures 4 and 5). In those areas the by-catches can exceed the 10% and 1% limits for cod and haddock respectively.

There is one other note of caution when considering a change in the position of the SMGL. Scott (1982) delineates the north and southwest edge of Sable Island Bank and the shallows around Sable Island Bank as areas rich in juvenile

haddock during the summer months. As there are very few adult haddock present in these areas Scott suggests these are specifically juvenile rearing areas. The selectivity pattern of the Scotian Shelf small meshed fishery is such that these juveniles being in close proximity to the silver hake fishery are vulnerable. In large catches of silver hake the juvenile haddock by-catch would be difficult to detect due to the similarity in shape, size, and colour when the two species are mixed together. Again such observations favour a status quo with regard to the present placement of the SMGL. It would not be advisable to move the SMGL further landwards.

The question of advancing the season to April 1 from April 15 hinges on by-catches of spawning haddock and not increased silver hake catches. Distributional patterns of haddock catches in the foreign silver hake fishery do not suggest by-catches of haddock in excess of 1% are a major problem (Figure 5, Table 14). In fact, pollock presents many more possible problems in that its by-catch can be expected to exceed 10% (Figure 6 and Table 20).

The advantages of opening the silver hake fishing season on April 1 would be that the fleets could optimize their catches at a time of year when the silver hake catch rates are high. This has the attractive possibility of the various foreign fleets attaining their quotas before the by-catches of haddock and cod become a major problem in July. The authors favour a limited experimental fishery to test this theory.

The possibility of managing by-catches on an overall fleet rather than the current situation on a per vessel basis is attractive for the fishery managers. It could reduce the difficulties of enforcing a per vessel by-catch limit. Often a vessel can be in a by-catch violation situation after one or two tows. Under a strict adherance to the regulations this vessel should leave the fishery. Policy now provides the Department of Fisheries and Oceans with the option to request the vessel to leave the area of high by-catch (this is often done).

The monitoring of individual vessel by-catches, admittedly difficult, does and has averted potentially damaging by-catch situations. During the 1982 silver hake fishery a group of five vessels encountered a large concentration of juvenile haddock. Tows of 7-10 tons of juvenile haddock were recorded by Canadian observers. Before it was possible for the Department of Fisheries and Oceans or the fleets to react to the situation, these vessels were in violation of the by-catch regulations. The fleet, however, was not. Continuation of this by-catch of juvenile haddock could have translated into a major problem for the future domestic haddock fishery. Instead, the Department of Fisheries and Oceans exercised its option and had the vessels move to another area. In a few days these vessels had caught enough silver hake to reduce the ratio of haddock to total catch below the 1% level and thus remain in the fishery.

This was a graphic example of the benefits of the current by-catch policy used by the Department of Fisheries and Oceans. The authors feel that this current mix of regulation and policy be maintained.

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Table 1. Catch of silver hake in NAFO Divisions 4VWX by Cuba and USSR.

Month	1977	1978	1979	1980	1981	1982*	1983*
1							
2							
3	3707						
4	8142	2118	2190	1558	618	2409	6990
5	5636	8760	13000	9777	13433	19482	16130
6	3026	13476	15264	13558	12375	24786	11133
7	10860	13906	12139	14474	13620	12450	542
8	2657	8838	2780	3894	829	93	
9	789	175	883	18	2	10	
10	112	193	375				
11	183	29	243	10			
12							
Total	35112	47495	46874	43269	40877	59230	34795

* Provisional

Table 2. Catch of silver hake in NAFO Divisions 4VWX by Japan.

Month	1977	1978	1979	1980	1981	1982*	1983*
1							
2							
3							
4							
5							
6	1	31					
7		12	122	10		157	
8	5	4	53	79	76	537	490
9	13	23	15	68	39	235	156
10		16	27	53	5		
11		20	2	29			
12		55					
Total	19	161	219	239	120	929	646

* Provisional

Table 3. Catch of squid (Illex) in NAFO Divisions 4VWX by Cuba and USSR.

Month	1977	1978	1979	1980	1981	1982*	1983*
1							
2							
3							
4	1						
5	809			9	1	7	
6	9877	77	2	50	38	22	3
7	9080	987	729	464	1650	182	3
8	1549	2714	1531	670	124	5	
9	940	2761	1026	396	17	1	
10		1735	73				
11		592	86				
12							
Total	22256	8866	3447	1589	1830	217	6

* Provisional

Table 4. Catch of squid in in NAFO Divisions 4VWX by Japan.

Month	1977	1978	1979	1980	1981	1982*	1983*
1							
2							
3							
4	2						
5	37						
6	74	9					
7	154	978	1277	395		95	
8	675	724	4054	5640	2876	108	357
9	1214	6029	10439	4798	2910	70	45
10	989	8342	8868	3864	273		
11		7533	2493	1978			
12		125	109				
Total	3145	23740	27240	16675	6059	273	402

* Preliminary

Table 5. Total finfish and squid catch (t) in 4VWX by Cuba and USSR reported to NAFO.

Month	1977	1978	1979	1980	1981	1982*	1983*
1							
2							
3							
4	2219	6514	2256	1927	13914	2410	7412
5	9565	6030	14684	12000	14595	19958	17278
6	14385	13098	16934	16000	14667	25260	12418
7	22734	19672	19292	19180	6637	13084	740
8	13495	4355	6907	7644	183	120	
9	3195	898	2672	544	17	24	
10	1988	133	537				
11	637	188	332	23			
12							
Total	68218	50888	63614	57318	50013	60856	37848

* Preliminary

Table 6. Total finfish and squid catch (t) by Japanese trawlers reported to NAFO.

Month	1977	1978	1979	1980	1981	1982*	1983*
1							
2							
3							
4	10						
5	51						
6	5	97					
7	351	1421	968	413		259	
8	911	1210	3370	6222	3056	727	1076
9	2160	5912	9841	5374	3190	426	279
10	1689	8713	7769	4381	298		6
11		7631	1683	2150			
12		215					
Total	5177	25199	23631	18540	6544	1412	1361

* Preliminary

Table 7. Catch (t) of cod in NAFO Divisions 4VWX by Cuba and USSR reported to NAFO.

Month	1977	1978	1979	1980	1981	1982*	1983*
1							
2							
3	44						
4	18	3		2			17
5	16	42	30	75	22	12	145
6		18	109	176	345	36	91
7	20	27	372	189	237	29	7
8	22	140	186	51	61		
9		4				1	
10							
11							
12							
Total	120	234	697	493	665	78	260

* Preliminary

Table 8. Observed cod by-catch rate (percent of total catch) for Cuba and USSR.

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4	1.48	.34		.15	.09	.03	.06
5	.49	.21	.28	.82	.32	.04	.41
6	.02	.14	.60	1.11	1.78	.10	1.03
7	.27	.62	1.05	.85	1.70	.56	1.68
8	.62	1.63	1.10	1.19	.48	1.50	
9	.18	.03	.04	.09		1.04	
10		.15	.09				
11		0	.03				
12							

* Preliminary

Table 9. Estimated cod catch (t) in 4VWX by Cuba and USSR.

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4	32.8	22.1		2.9	12.5	.7	4.5
5	46.9	12.7	41.2	98.4	46.7	8.0	70.8
6	2.9	18.3	101.7	177.6	261.1	25.3	127.9
7	61.4	122.0	202.6	163.0	112.8	73.3	12.4
8	83.7	71.0	76.3	91.0	.9	1.0	
9	5.8	.3	1.1	.5		.2	
10		.2	.5				
11			.1				
12							
Total	233.4	246.6	423.5	533.4	434.0	108.5	215.6

Table 10. Catch of cod (t) in NAFO Divisions 4VWX by Japan reported to NAFO.

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4							
5							
6							
7				1		1	
8		1	1	5			4
9		2		2	2	1	
10		2		2	3		
11		3	1	4			
12		3					
Total		11	2	14	5	2	4

Table 11. Cod by-catch rate (percent of total catch) for Japan fishing in NAFO Divisions 4VWX (from observer data).

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4							
5							
6							
7	.27	.63	1.05	.25			
8	.62	1.63	1.10	.07	.06	0	.46
9	.18	.05	.01	.08	.15	.12	.27
10		.15	.02	.04	1.30		
11		1.10	.01				
12							

Table 12. Estimated cod catch (t) in 4VWX by Japan.

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4							
5							
6							
7		.4	1.1	1.0			
8	.2	1.0	1.7	4.4	1.8		
9	1.7	.6	1.0	4.3	4.8		5.0
10		8.7	1.6	1.8	3.0	.5	.7
11		81.7	.2				
12							
Total	1.9	92.4	5.5	11.5	9.6	.5	5.7

Table 13. Catch (t) of haddock in NAFO Divisions 4VWX by Cuba and USSR reported to NAFO.

Month	1977	1978	1979	1980	1981	1982*	1983*
1							
2							
3	6						
4	20						17
5	4	7	50	61	27	24	98
6	2	28	38	77	135	35	153
7		54	21	126	37	33	24
8		137	8	31	2	2	
9	1	5	10				
10		6					
11	1						
Total	34	237	127	295	201	94	292

* Preliminary

Table 14. Haddock by-catch rate (percent of total catch) for Cuba and USSR fishing in NAFO Divisions 4VWX. (from observer data).

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4	4.12	.14		.08	.07	.02	.12
5	6.54	.34	.58	.52	.31	.33	.80
6	.25	.54	.55	.63	1.67	.13	2.32
7	.18	.82	.24	.73	.58	.82	33.28
8	.29	1.78	.34	1.32	.13	.33	
9	.14	.25	.05	.10		10.67	
10		.11	.57				
11		1.07	.14				
12							

Table 15. Estimated haddock catch in 4VWX by Cuba and USSR.

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4	91.4	9.1		1.5	9.7	.5	8.9
5	625.6	20.5	80.8	62.4	45.2	65.9	138.2
6	36.0	70.7	93.1	100.8	244.9	32.8	288.1
7	40.9	161.3	46.3	140.0	38.5	107.3	246.3
8	39.1	77.5	23.5	100.9	.2	.4	
9	4.5	2.2	1.3	.5		2.6	
10		.1	3.1				
11			.5				
12							
Total	837.5	341.6	248.5	406.2	338.7	209.4	581.5

Table 16. Catch of haddock - NAFO Divisions 4VWX by Japan.

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4							
5							
6		9					
7		6	5	2			
8		3	2	7	10	6	42
9	1	6	3	16	8	6	5
10		6	8	4	4		
11		6	1	8			
12		14					
Total	1	50	19	37	22	12	47

Table 17. Haddock by-catch rate (percent of total catch) for Japan fishing in NAFO Divisions 4VWX (from observer data).

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4							
5							
6							
7	.01	.18	.73	.56			
8	.09	.37	.06	.31	.54	1.18	4.53
9	1.07	.03	.02	.36	.53	1.47	2.33
10		.07	.12	.19	2.27		
11		.30	.03				
12							

Table 18. Estimated haddock catch in 4VWX by Japan.

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4							
5							
6							
7	0	2.6	7.1	2.3			
8	.8	4.5	2.0	19.3	16.5	8.6	48.7
9	23.1	3.5	2.0	19.3	16.9	6.3	6.5
10		6.1	9.3	8.3	6.8		
11		22.9	.5				
12							
Total	24.0	39.6	20.9	49.3	40.2	14.8	55.2

Table 19. Catch of pollock in NAFO Divisions 4VWX by Cuba and USSR.

Month	1977	1978	1979	1980	1981	1982*	1983*
1							
2							
3							
4	43	9	10	127	47		16
5	91	109	704	546	114	193	352
6	2	193	226	264	108	113	107
7	11	189	101	43	80	75	12
8		135	5	2	9		
9		5	26				
10							
11							
12							
Total	147	640	1072	982	358	381	487

* Preliminary

Table 20. Pollock by-catch rate (percent of total catch) for Cuba and USSR fishing in NAFO Divisions 4VWX (from observer data).

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4	7.25	.16		6.39	.59		.62
5	1.69	.83	5.15	3.86	.07	1.26	2.21
6	.02	1.20	1.11	1.44	.38	.52	2.23
7	.03	.96	.27	.35	.01	.55	1.92
8	.11	1.21	.08	.14	.01	.01	
9	.02	.05	.60	.01	.01	.05	
10		.02	.27				
11		0	1.91				
12							

Table 21. Estimated pollock catch in 4VWX by Cuba and USSR.

Month	1977	1978	1979	1990	1981	1982	1983
1							
2							
3							
4	160.9	10.4		123.1	82.1		46.0
5	161.6	50.0	756.2	463.2	10.2	251.5	381.8
6	2.9	157.2	188.0	230.4	55.7	131.4	276.9
7	6.8	188.9	52.1	67.1	.7	72.0	14.2
8	14.8	52.7	5.5	10.7			
9	.6	.4	14.2	.1			
10			1.4				
11			6.3				
12							
Total	347.7	459.7	1023.8	894.6	148.7	454.8	718.9

Table 22. Catch of pollock in NAFO Divisions 4VWX by Japan.

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4							
5							
6		15					
7		1		4			
8				12	5	1	6
9	1	1	15	9	8	1	
10		89	3	27	2		
11		2	1	29			
12		2					
Total	1	110	19	81	15	2	6

Table 23. Pollock by-catch rate (percent of total catch) for Japan fishing in NAFO Divisions 4VWX (from observer data).

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4							
5							
6							
7	0	.11	.03	.31		.	
8	0	.05	.02	.27	.32	.25	.60
9	.16	.04	.19	.21	.29	.04	.02
10		.65	.04	.42	.17		
11		.09	.03				
12							

Table 24. Estimated pollock catch in 4VWX by Japan.

Month	1977	1978	1979	1980	1981	1982	1983
1							
2							
3							
4							
5							
6							
7		1.6	.3	1.3			
8		.6	.7	16.8	9.8	1.8	6.5
9	3.5	2.4	18.7	11.3	9.3	.2	
10		56.6	3.1	18.4	.5		
11		6.9	.5				
12							
Total	3.5	68.0	23.3	47.8	19.5	2.0	6.5

Table 25. Foreign removals at age (numbers '000) for 4VsW cod.

Age	1978	1979	1980	1981	1982
1	29	12	31	3	5
2	63	46	25	63	1
3	152	106	83	302	12
4	178	214	73	281	16
5	27	181	92	41	10
6	3	42	45	25	3
7	1	5	17	8	1
8	0	0	1	1	0
9	0	0	1	0	0
Sum 1+	453	606	368	724	48

Table 26. Foreign removals at age (numbers '000) for 4VW haddock.

Age	1977	1978	1979	1980	1981	1982
1	37	141	1	1015	1289	805
2	69	158	135	72	143	330
3	55	211	147	203	19	44
4	7	119	153	41	60	6
5	12	7	43	19	32	24
6	4	7	6	6	14	12
7	1	2	4	1	2	3
8	1	0	1	1	0	0
9	0	0	0	0	1	1
10	0	0	1	0	0	0
11	0	0	0	0	0	0
Sum 1+	186	645	492	1358	1560	1225

Table 27. Partial fishing mortality for the domestic fishery of 4VsW cod.

Age	1978	1979	1980	1981	1982
1	0.001	0.000	0.000	0.000	0.000
2	0.001	0.000	0.002	0.004	0.000
3	0.015	0.031	0.025	0.054	0.030
4	0.082	0.142	0.136	0.160	0.139
5	0.204	0.315	0.249	0.294	0.250
6	0.312	0.344	0.369	0.271	0.250
7	0.260	0.239	0.361	0.311	0.250
8	0.199	0.192	0.362	0.309	0.250
9	0.130	0.101	0.252	0.219	0.250

Table 28. Partial fishing mortality for the domestic fishery of 4VW haddock.

Age	1977	1978	1979	1980	1981	1982
1	0.000	0.000	0.000	0.000	0.000	0.000
2	0.003	0.000	0.000	0.000	0.003	0.001
3	0.060	0.032	0.014	0.052	0.040	0.065
4	0.191	0.264	0.066	0.246	0.159	0.172
5	0.258	0.316	0.117	0.293	0.525	0.272
6	0.413	0.672	0.176	0.520	0.503	0.398
7	0.483	0.788	0.256	0.479	0.900	0.399
8	0.595	0.704	0.095	0.396	0.564	0.399
9	0.371	0.567	0.120	0.460	0.676	0.394
10	0.841	0.441	0.135	0.309	0.782	0.400
11	0.453	0.626	0.176	0.504	0.578	0.400

Table 29. Projected potential domestic catch (t) of the 1978-1982 USSR catch of 4VsW cod. USSR catch weights were estimated using observed by-catch rates and total finfish reported catches.

Year	USSR Catch (t)	Potential Domestic Catch (t)	Cumulative Domestic Catch (t)
1978	375	28	28
1979	679	221	249
1980	421	332	581
1981	653	388	969
1982	45	345	1314
1983		290	1604
1984		253	1857
1985		205	2062
1986		161	2223
1987		118	2341
1988		81	2422
1989		54	2476
1990		37	2513
1991+		67	2580

Table 30. Projected potential domestic catch (t) of the 1977-1982 foreign catch of 4VW haddock. Foreign catch weights were estimated using observed by-catch rates and reported silver hake catches.

Year	Foreign Catch (t)	Potential Domestic Catch (t)	Cumulative Domestic Catch (t)
1977	134	14	14
1978	265	67	81
1979	425	61	142
1980	325	260	402
1981	304	350	752
1982	280	254	1006
1983		211	1217
1984		282	1499
1985		348	1847
1986		363	2210
1987		316	2526
1988		245	2771
1989		183	2954
1990		133	3087
1991+		242	3329

Table 31. Long-term projection of potential gains in catch (t) to the 4VW haddock fishery if the foreign fleets raised their codend mesh size from 60 mm to 130 mm.

Year	Projected Catch at $F_{0.1}$ = .220 and Current Mesh Sizes	Projected Catch at $F_{0.1}$ = .224 and Foreign Fleet at 130 mm	Potential Increase in Yield to the Total Fishery	Cumulative Increase in Yield to the Total Fishery
1984	11660	11711	51	51
1985	14529	14690	161	212
1986	16224	16412	188	400
1987	16151	16341	190	590
1988	14586	14745	159	749
1989	12310	12455	145	894
1990	10886	11008	122	1016

Table 32. Long-term projection of potential gains in catch (t) to the 4VsW cod fishery if the foreign fleets increased their codend mesh sizes from 60 to 130 mm.

Year	Projected Catch (t) at F0.1 = .1623 and Current Codend Mesh Sizes	Projected Catch (t) at F0.1 = .1621 Foreign Fleet at Codend Mesh Size	Potential Increase in Yield to the Total Fishery	Cumulative Increase in Yield to the Total Fishery
1984	52975	52924	-51	-51
1985	58501	58461	-40	-91
1986	62879	62852	-27	-118
1987	65960	65944	-16	-134
1988	68192	68184	-8	-142
1989	69685	69684	-1	-143
1990	70917	70923	6	-137

Table 33. Silver hake Thompson and Bell Y/R calculations.

Age	1	2	3	4	5	6	7	8	9	10
Wt (kg)	0.051	0.140	0.202	0.263	0.322	0.387	0.522	0.683	0.844	0.923
60mm PR.	0.028	0.248	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
*90mm PR.	0.024	0.174	0.678	0.674	0.711	0.838	1.000	1.000	1.000	1.000
** Sel 90/60	0.850	0.703	0.678	0.674	0.711	0.838	1.000	1.000	1.000	1.000

Assume 60 mm Gear

	Fishing Mortality	Catch (Number)	Yield (kg)	Avg. Weight (kg)	Yield Per Unit Effort
	0.300	0.22394	0.054	0.241	1.000
$F_{0.1}$	0.413	0.26879	0.060	0.224	0.802
	0.600	0.31857	0.066	0.207	0.611
	0.900	0.37292	0.071	0.190	0.438
	1.200	0.40951	0.073	0.179	0.340
	1.500	0.43662	0.075	0.171	0.277
	1.800	0.45801	0.076	0.165	0.234
	2.100	0.47564	0.076	0.160	0.202
	2.400	0.49065	0.077	0.156	0.177
	2.700	0.50373	0.077	0.152	0.158
	3.000	0.51534	0.077	0.149	0.142
F_{MAX}	3.119	0.51961	0.077	0.148	0.137
	3.300	0.52579	0.077	0.146	0.129

* $PR_{90} = PR \times Sel_{90}/Sel_{60}$

** Clay, 1979a

Table 33. Continued.

Assume 90 mm Gear

	Fishing Mortality	Catch (Number)	Yield (kg)	Avg. Weight (kg)	Yield Per Unit Effort
	0.400	0.21642	0.053	0.245	1.000
$F_{0.1}$	0.568	0.26242	0.060	0.227	0.792
	0.800	0.30884	0.065	0.210	0.612
	1.200	0.36342	0.070	0.192	0.439
	1.600	0.40085	0.073	0.181	0.342
	2.000	0.42884	0.074	0.173	0.279
	2.400	0.45101	0.075	0.166	0.236
	2.800	0.46932	0.076	0.161	0.204
	3.200	0.48490	0.076	0.157	0.179
	3.600	0.49847	0.076	0.153	0.160
	4.000	0.51051	0.076	0.149	0.144
F_{MAX}	4.252	0.51745	0.076	0.147	0.135
	4.400	0.52134	0.076	0.146	0.131
	4.800	0.53119	0.076	0.143	0.120
	5.200	0.54023	0.076	0.141	0.110
	5.600	0.54860	0.076	0.138	0.102
	6.000	0.55638	0.076	0.136	0.095
	6.400	0.56367	0.076	0.134	0.089
	6.800	0.57051	0.075	0.132	0.084
	7.200	0.57697	0.075	0.130	0.079
	7.600	0.58308	0.075	0.129	0.075
	8.000	0.58888	0.075	0.127	0.071

Table 34. Long-term projections of yields to the 4VWX silver hake small meshed fishery if the current codend mesh size of 60 mm is increased to 90 mm in 1983 to 1990.

Year	Projected Catch at $F_{0.1}$ = .418 for 60 mm	Projected Catch at $F_{0.1}$ = .568 for 90 mm	Increase in Yield (t) at $F_{0.1}$ for 60 to 90 mm	Cumulative Increase in Yield (t) at $F_{0.1}$ for 60 to 90 mm
1982	53428	53428	-	-
1983	92156	90987	-1169	-1169
1984	89830	89100	-730	-1899
1985	82773	81934	-839	-2738
1986	78663	77766	-897	-3635
1987	76506	75869	-637	-4272
1988	75249	74684	-565	-4837
1989	74685	74048	-637	-5474
1990	74562	73874	-688	-6162

Table 35. Projected beginning of the year population biomass ('000 t) of 4VWX silver hake fishery for 60 and 90 mm codends at constant effort ($F_{0.1}$ = .418) and increased effort ($F_{0.1}$ = .568) in 1983 to 1990.

Year	$F_{0.1}$ = .418 60 mm	$F_{0.1}$ = .568 90 mm	Change Constant Effort (60-90)	Change Increased Effort (60-90)	Cumulative Change Constant Effort	Cumulative Change Increased Effort
1982	490	490	-	-	-	-
1983	516	516	-	-	-	-
1984	486	487	22	1	22	1
1985	461	462	35	1	57	2
1986	446	448	41	2	98	4
1987	439	441	43	2	147	6
1988	435	438	44	3	183	9
1989	432	436	44	4	229	13
1990	431	435	44	4	273	17

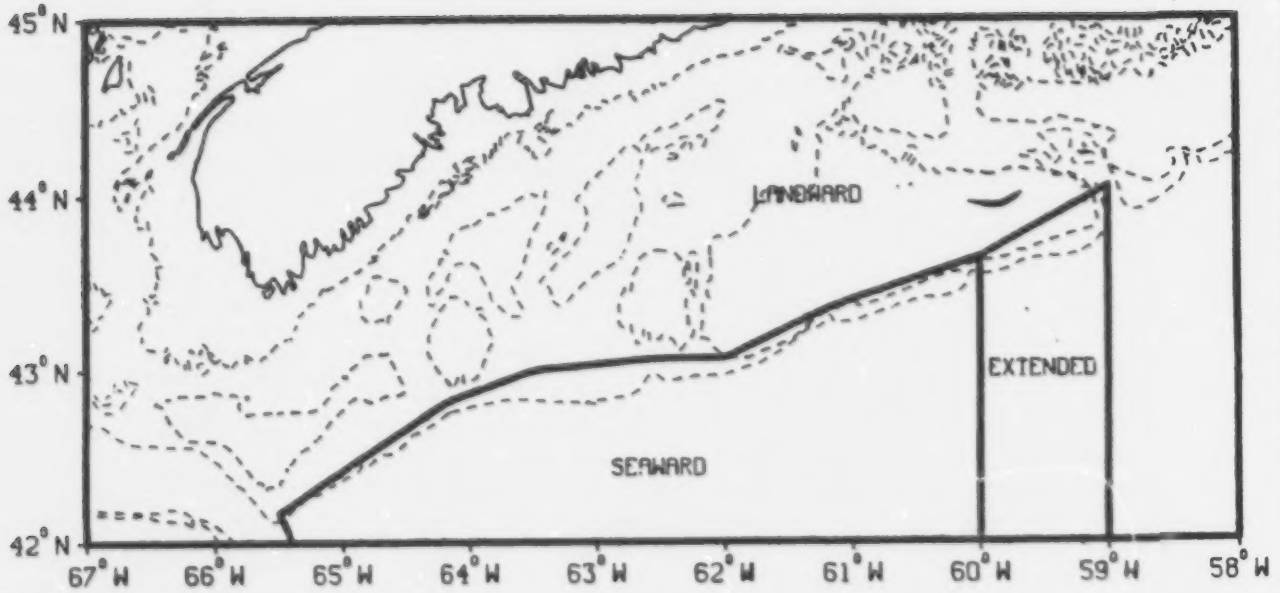


Figure 1. The Scotian Shelf small meshed gear line (SMGL) used to manage the the silver hake, squid and argentine fisheries.

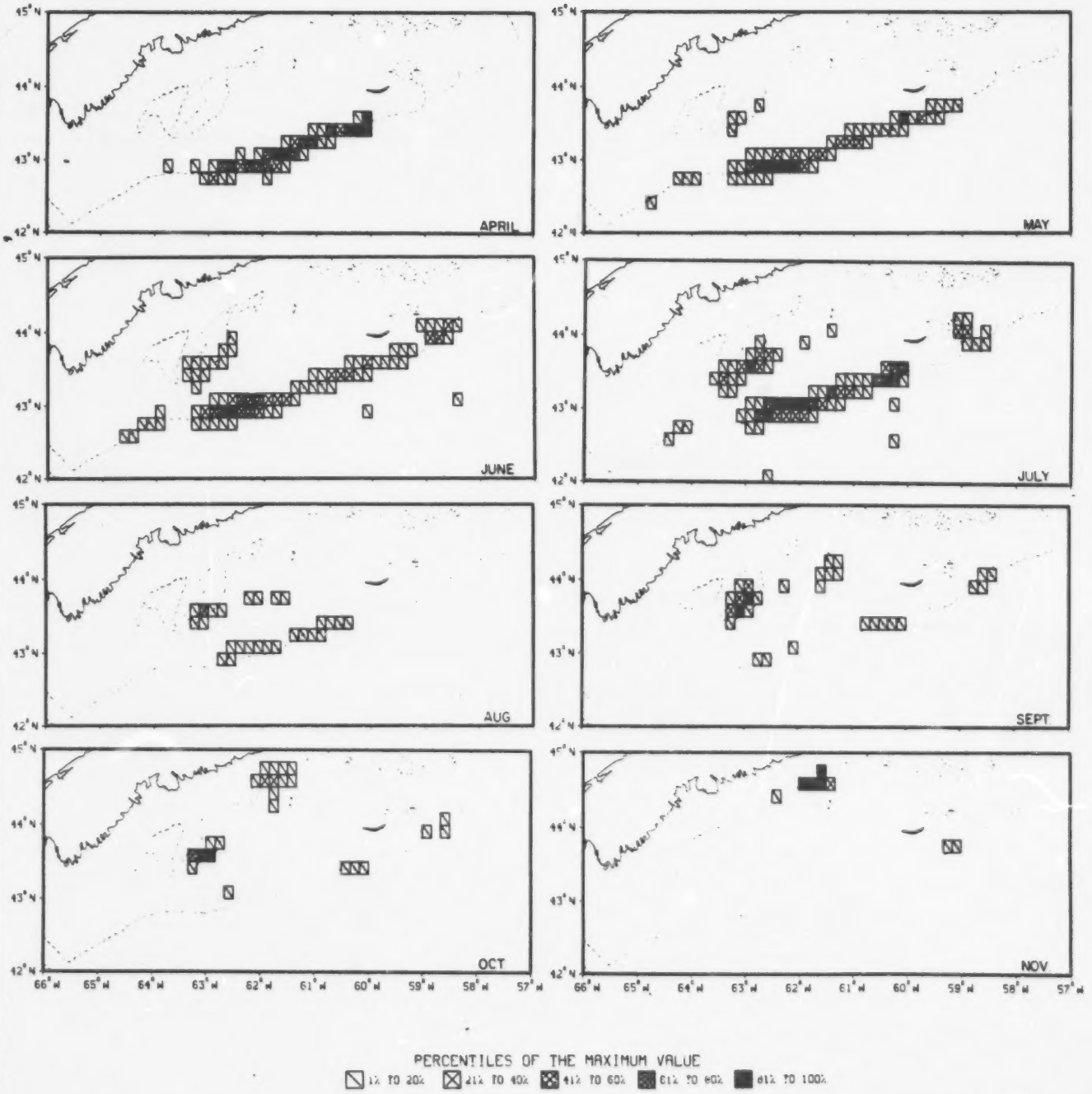


Figure 2. Seasonal distribution of observed silver hake catches from the small meshed fishery aggregated by ten minute squares. Shading is done in proportion to the maximum aggregated value.

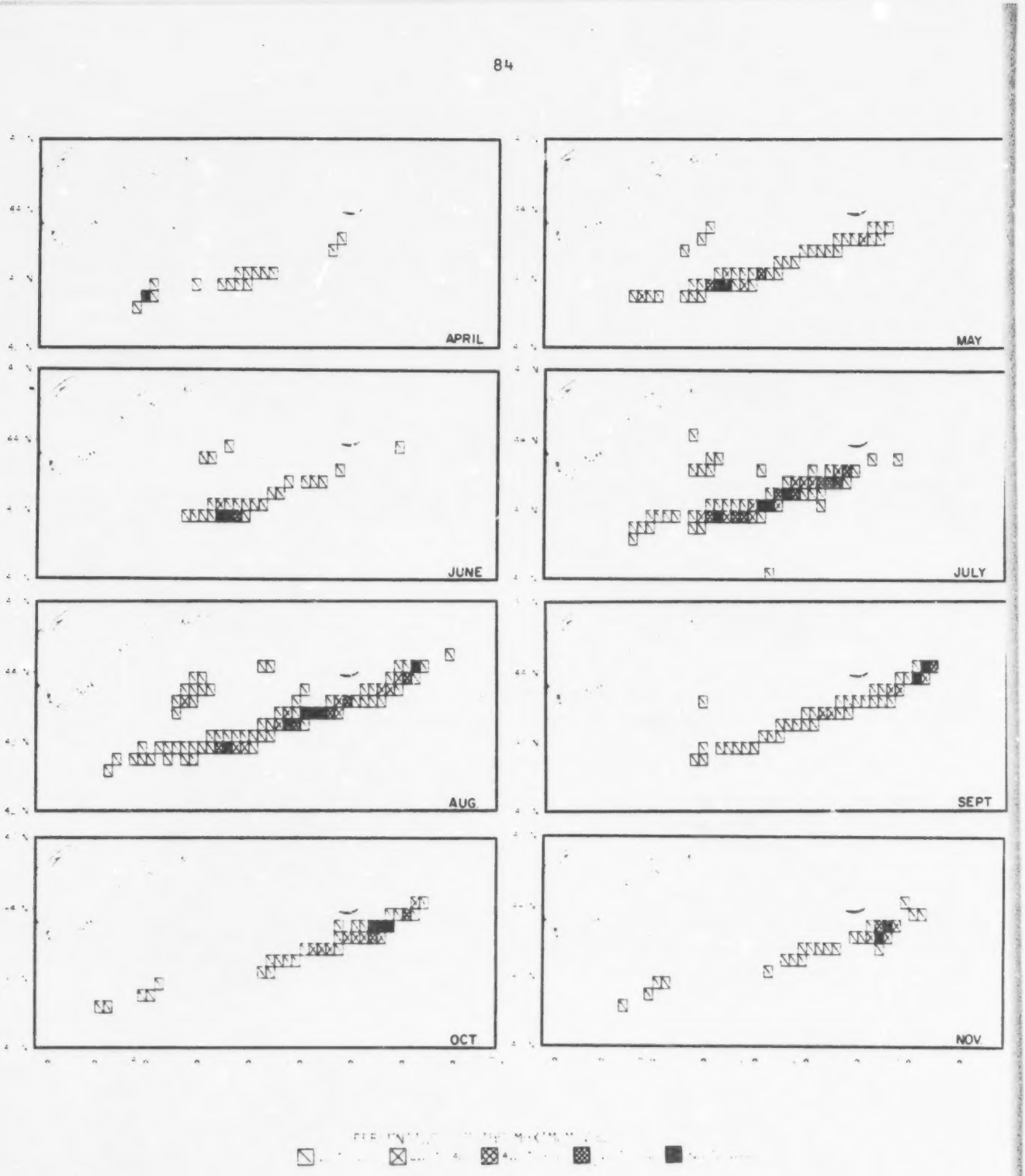


Figure 3. Seasonal distribution of observed squid catches from the Cuban, Japanese and Soviet small meshed fishery aggregated by ten minute squares. Shading is done in proportion to the maximum aggregated value.

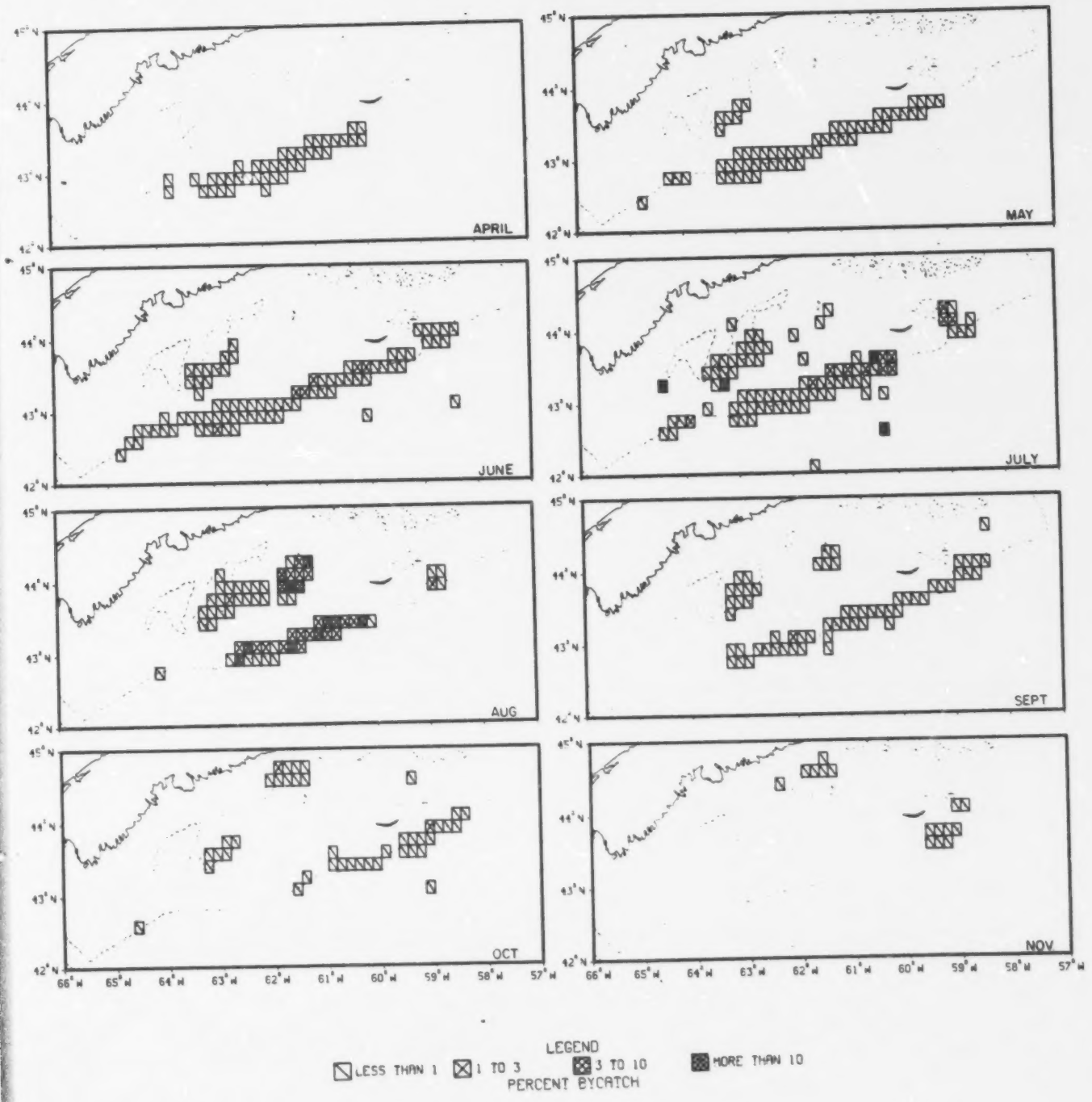


Figure 4. Seasonal distribution of observed cod by-catch from the small meshed fishery aggregated by ten minute squares. Shading indicates by-catch levels.

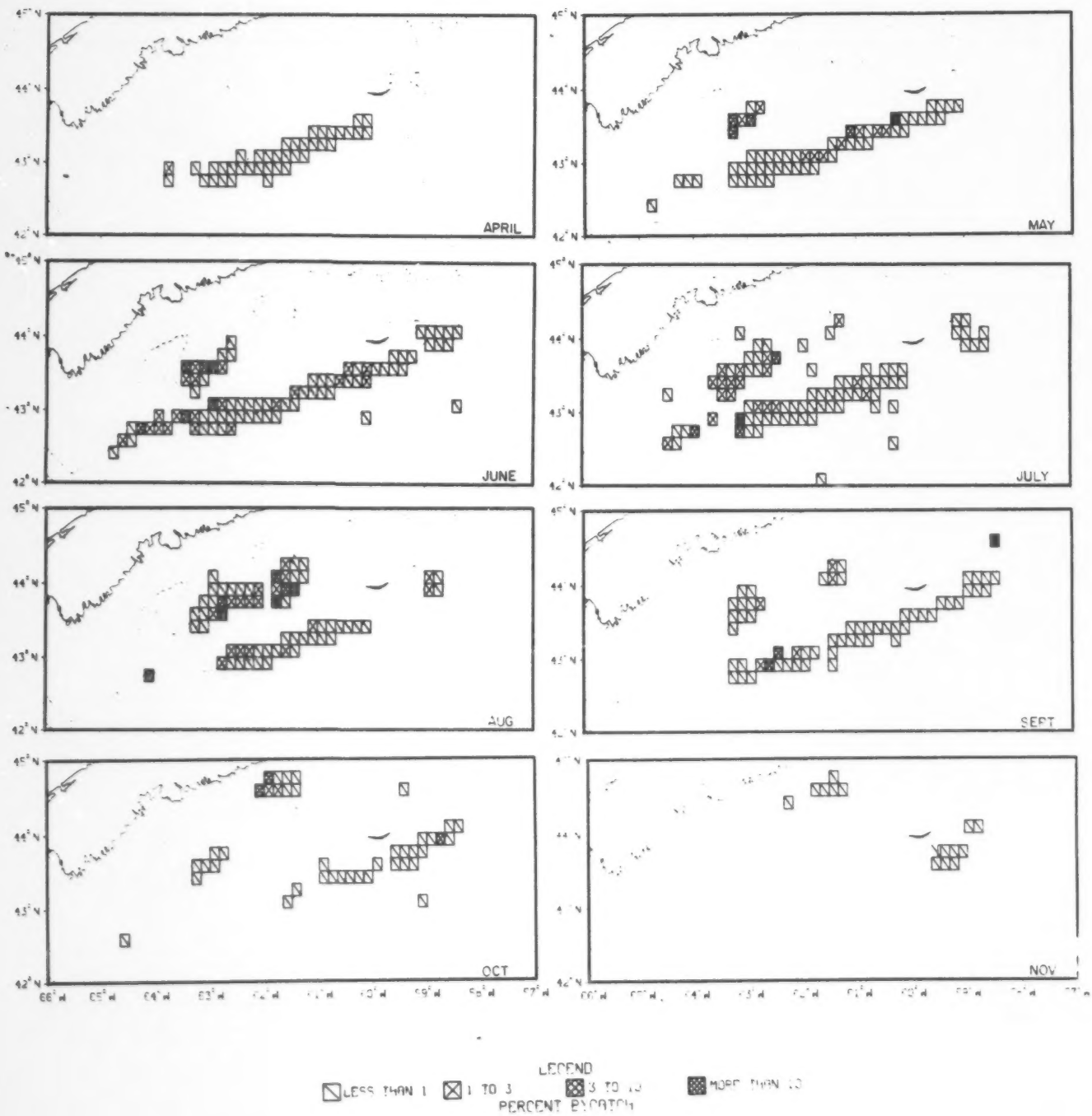


Figure 5. Seasonal distribution of observed haddock by-catch from the small meshed fishery aggregated by ten minute squares. Shading indicates by-catch levels.

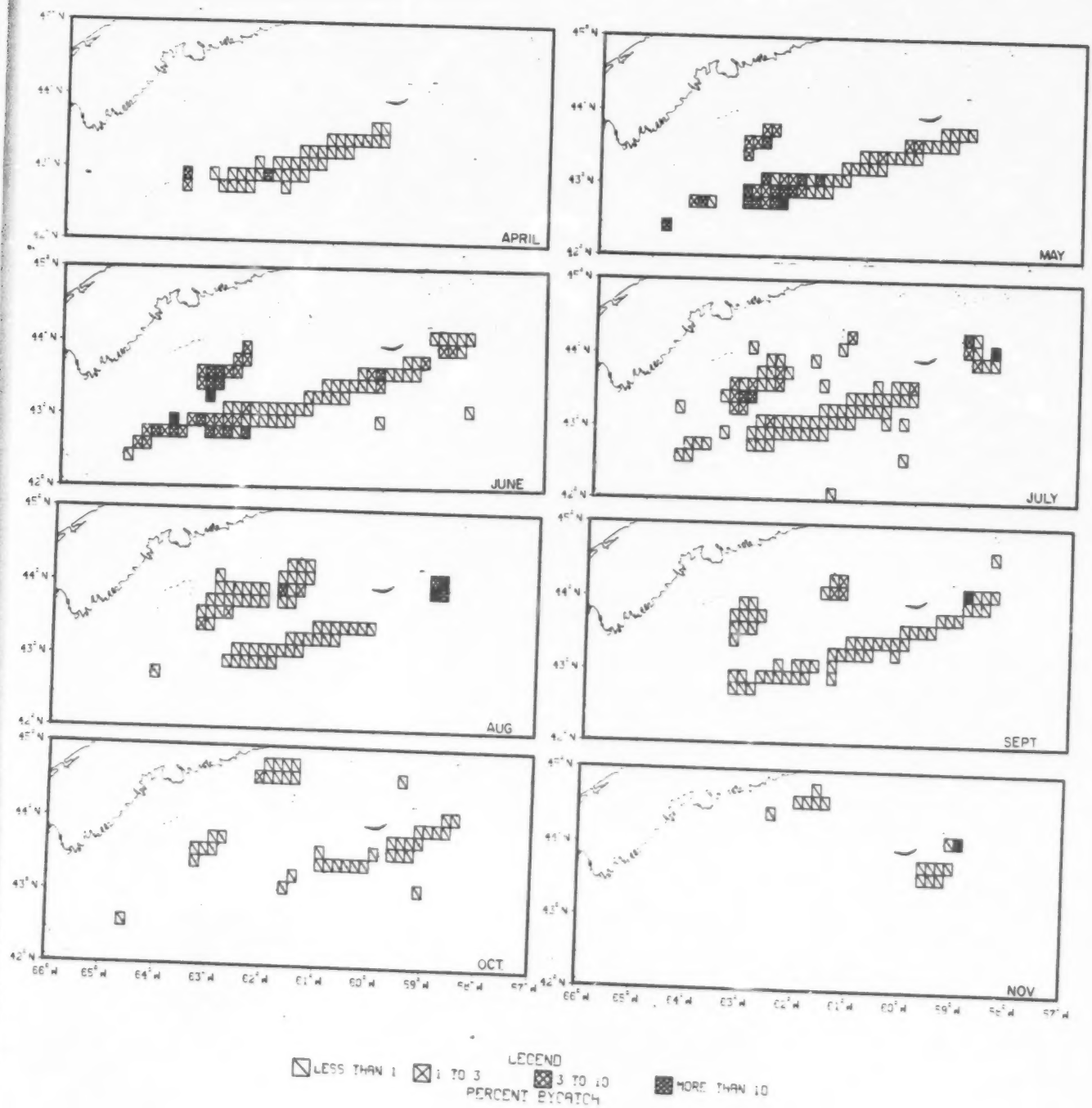


Figure 6. Seasonal distribution of observed pollock by-catch from the small meshed fishery aggregated by ten minute squares. Shading indicates by-catch levels.

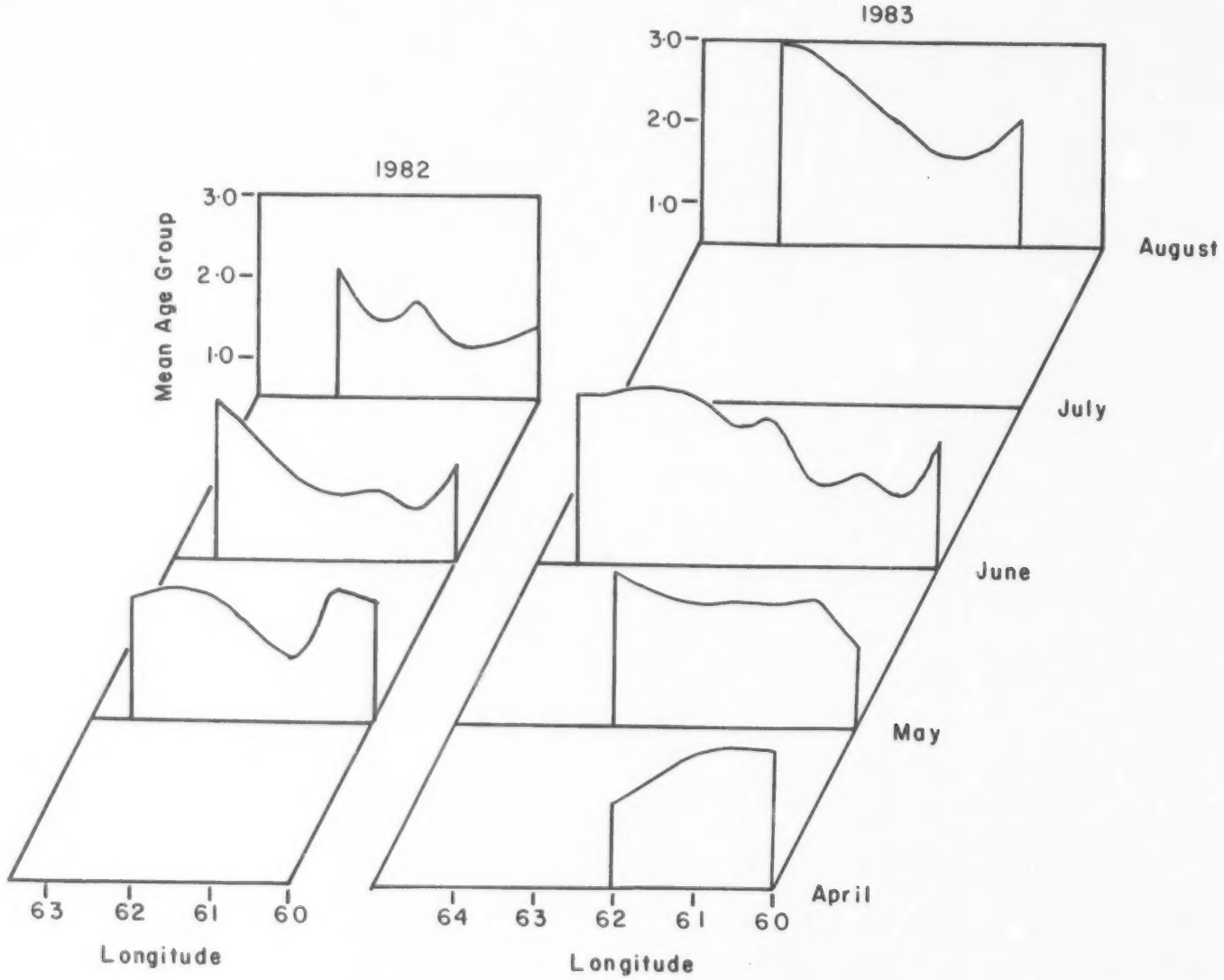


Figure 7. Mean age by longitude and month of haddock sampled from the Scotian Shelf small meshed fishery in 1982 and 1983.

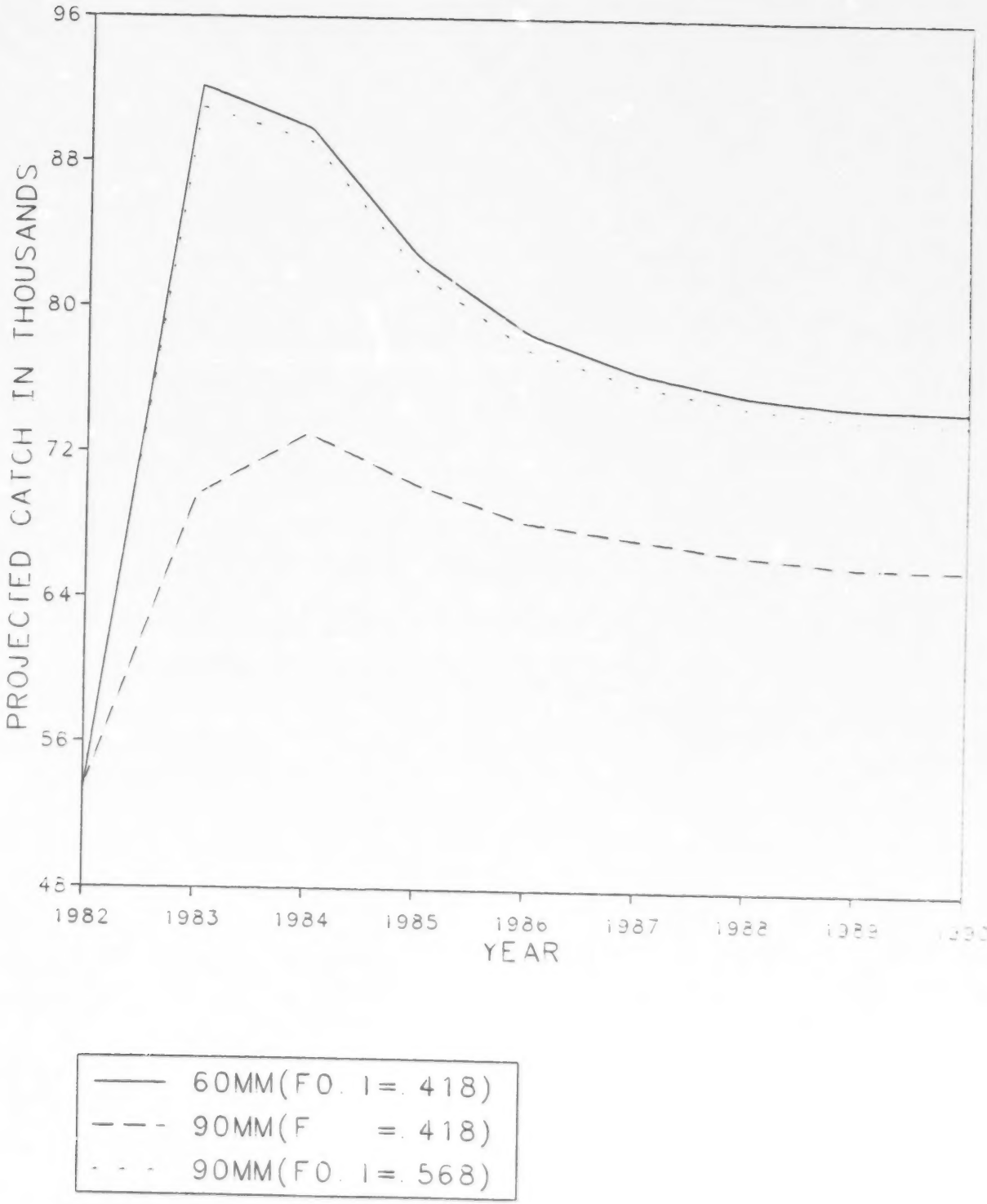


Figure 8. Projected small meshed catches (t) of Scotian Shelf silver hake for 60 mm, and 90 mm. codend selectivities.

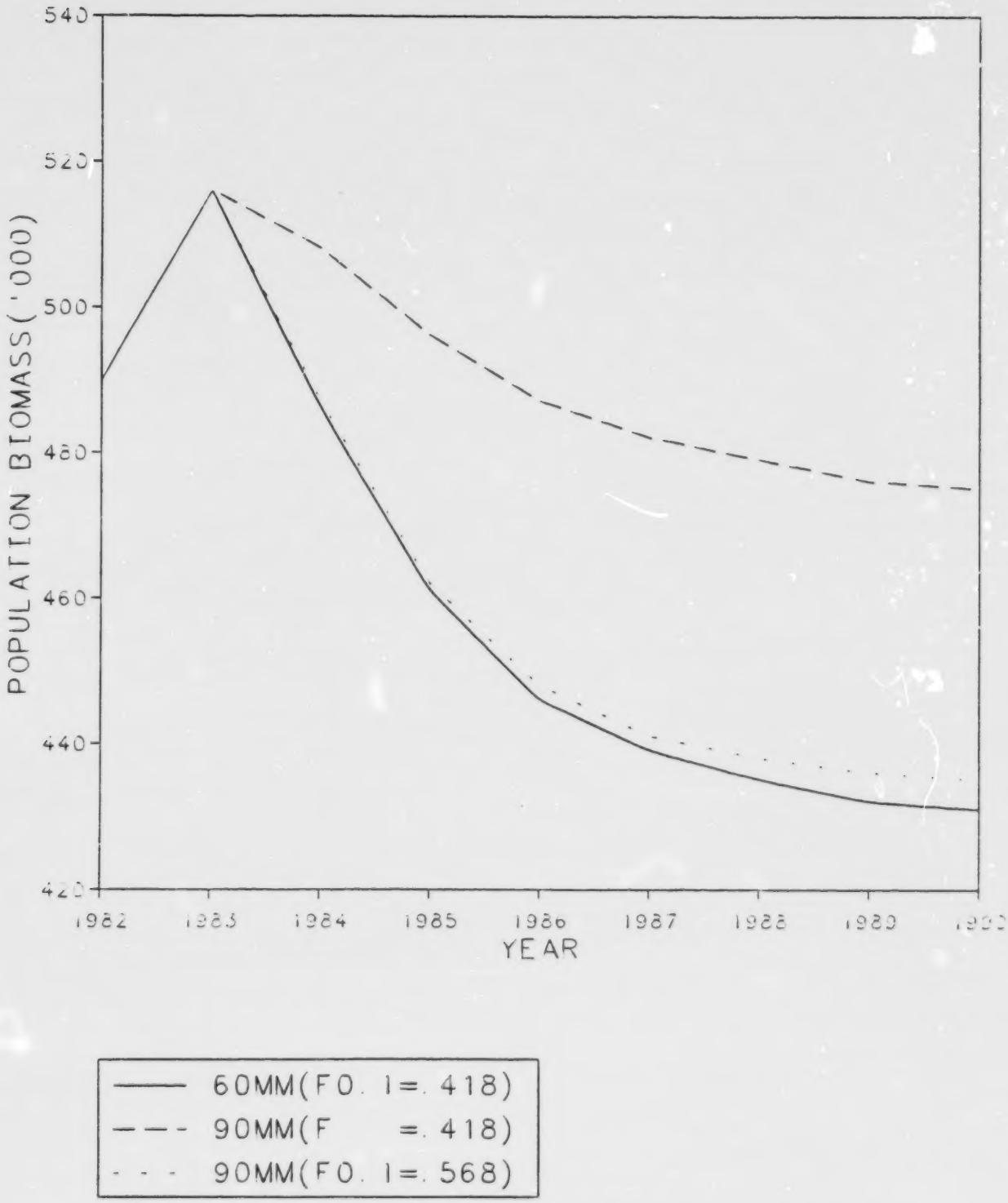
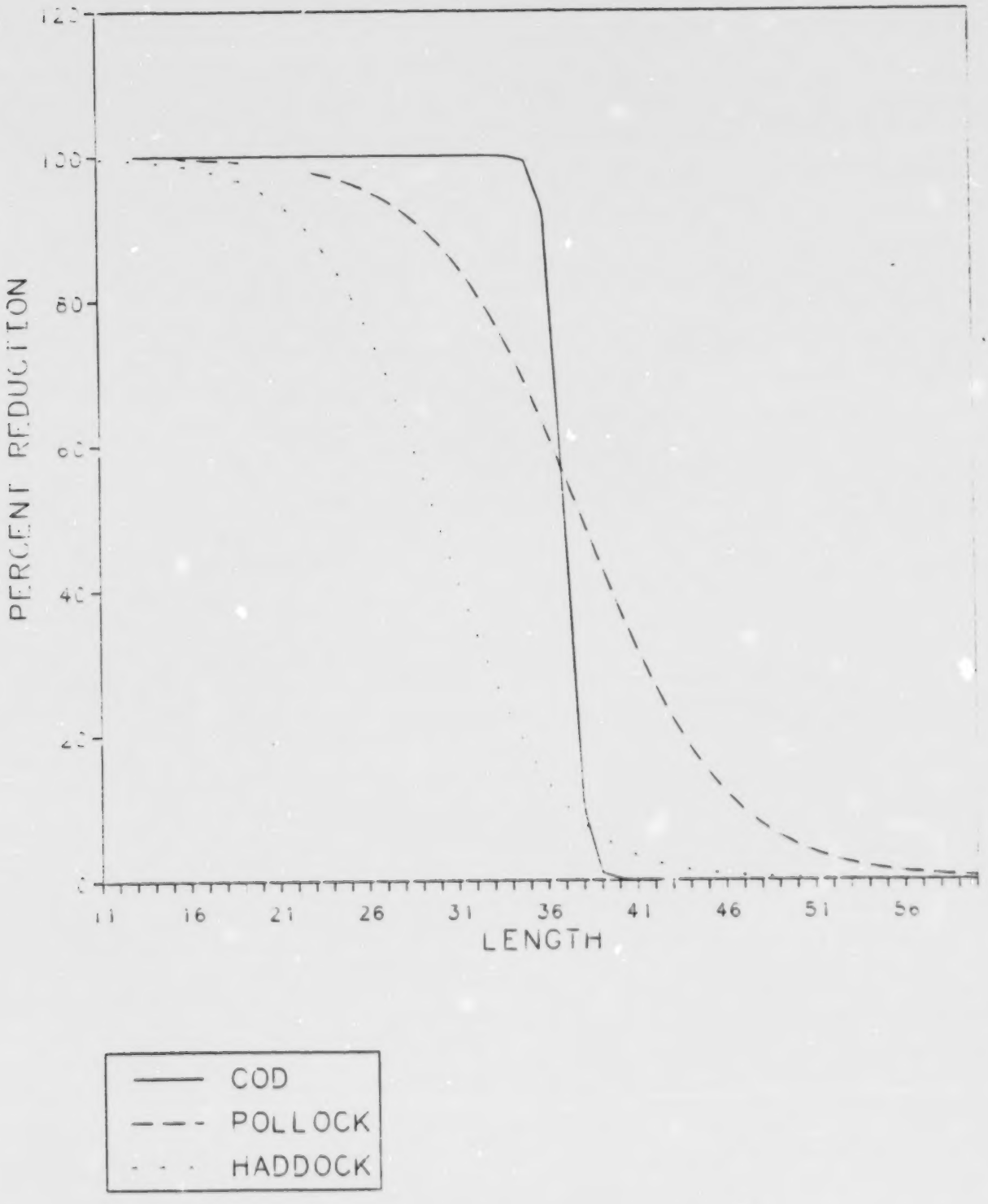


Figure 9. Projected beginning of the year population biomass (t) of Scotian Shelf silver hake fishing at different levels of F and with different mesh sizes.



gure 10. Percentage reductions of observed cod, haddock and pollock catch numbers at length if the small meshed fishery used 90 mm rather than 60 mm codends.

A Linear Programming Analysis of Scotian Shelf Offshore Fisheries

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Linear Programming (LP) was used to analyse the allocation of 11 catch quotas on the Scotian Shelf and Georges Bank among the Canadian offshore otter trawler fleet (mobile gear greater than 100'). The LP equations were formulated in terms of catch per unit effort and the solutions were in terms of fishery specific effort levels. Three objective functions were used: maximize total catch, maximize total revenue, and maximize net revenue. Input data were obtained from the Scotia-Fundy International Observer Program for the years 1982 and 1983. Two catch quotas were consistently not taken in the LP analyses; namely the Division 4X cod and the Subarea 5 haddock. Optimal total effort levels from the LP analyses indicated that the existing offshore fleet was probably not too large for the available quotas. However, there was little agreement between the observed effort distributions among fisheries and the optimal effort distributions from LP analyses.

Introduction

Fisheries management in the Atlantic Canadian groundfish fisheries relies on single-species total allowable catches (TACs) to control fishing mortality, stock size, and long term yields. Individual stock sizes are assessed and optimal TACs are determined based on harvesting objectives. These TACs are then allocated among various fleet components (e.g. vessel classes) in an attempt to ensure the viability of these components. The allocations are referred to as quotas. Catch by species and fleet component is monitored throughout the fishing year with the intent of closing individual fisheries when the quotas are reached.

A major problem with this single-species approach to management is that the fisheries yield catch from more than one species at a time. Each species is either caught as the main species in the fishery, i.e. the species to which the fisherman directs his effort, or as an incidental catch or by-catch in another fishery. If one is to strictly enforce quotas, all fisheries which yield a certain species would have to be closed when the quota for that species is reached. This could lead to substantial losses in total yield from the interacting fisheries if all the fisheries are constrained by one relatively small species-quota.

This type of problem has been analysed using linear programming (LP) techniques (Brown et al. 1973, 1978; Brodie 1981; Murawski et al. 1983). The basic form of the problem is to maximize (or minimize) an objective function, which is a linear function of a series of unknowns, subject to a set of constraints which are linear equalities (or inequalities) in the unknowns (Luenberger 1973). In the fisheries example a possible objective may be to maximize total catch of all species subject to the constraints of individual species quotas. The optimal solution may consist of a series of fishery specific effort levels (f_i) which maximize:

$$\sum_{i=1}^{m} \sum_{j=1}^{n} S_{ij} f_i$$

Subject to:

$$\sum_{i=1}^{m} S_{ij} f_i \leq Q_j \qquad (1)$$

where: S_{ij} = the catch per unit effort of species j in fishery i

Q_j = the quota for speices j

The LP technique offers a wide variety of possible formulations. The objectives may be stated in economic terms such as to maximize total or net revenues from fishing (Anderson 1975). Murawski et al. (1983) stated the constraints in terms of fishing mortality levels. Brown et al. (1973, 1978) solved for directed species catch levels rather than fishery specific effort levels. The precise formulations used is dependent upon the specific type of problem being analysed and the desired objective.

The Canadian offshore groundfish fisheries on the Scotian Shelf (Division 4X to Subdivision 4Vn) and Georges Bank (Subdivision 5Ze) (Figure 1) are analysed in this paper to determine if any single-species quotas may be restrictive. The fishery equations are defined in terms of catch per unit effort so that the optimal solutions are in terms of fishery specific effort levels. These are compared to observed fleet effort distribution and fleet size. Three specific objective functions are used for comparative purposes: maximize total catch, maximize total revenues, and maximize net revenues.

Methods

Eleven groundfish species TACs are fished by Canadian offshore vessels (greater than 100') in the Scotian Shelf and Georges Bank area; five cod (Gadus morhua), three haddock (Melanogrammus

aeglefinus), and one each for redfish (Sebastes sp.), pollock (Pollachius virens), and flounder. The flounder group consists of American plaice (Hippoglossoides platesoides), yellowtail (Limanda ferruginea), and witch (Glyptocephaus cynoglossus). The TACs apply to specific management units comprising combinations of fish stock (or stocks), area, and time period. The geographic size of these management units varies considerably. For example the pollock management unit includes NAFO Divisions 4VWX and Subarea 5 while one cod management unit includes only Subdivision 4Vn (Figure 1). Furthermore this latter management unit applies to catches for the months of May to December. The 11 management units are described in Table 1. One of these, the Subdivisions 4Vn cod (May to December) is not actively fished by the offshore sector on a regular basis and it was not included in this analysis. The remaining 10 quotas formed the constraints for the LP analysis.

An analysis of data collected by the Scotia-Fundy Region International Observer Program (Kulka and Waldron 1983) on Canadian offshore groundfish vessels fishing on the Scotian Shelf and Georges Bank in 1982 and 1983 revealed six distinct "fisheries" based on similarities in catch composition (Sinclair 1985). Their species compositions are shown in Figure 2. Four fisheries were specific for cod, haddock, redfish, and pollock, respectively with greater than 80% of the total catch being comprised of one species. The other two were of mixed species composition, one was of flounder and cod and the other of cod, haddock, and pollock. The cod fishery dominated the shallow waters of the eastern Scotian Shelf along with the mixed flounder and cod fishery. The haddock and pollock fisheries dominated the shallow areas of the central and western Scotian Shelf. The redfish fishery was found in the deep areas along the Scotian Shelf edge and in the deep holes on the Shelf. The mixed cod, haddock, and pollock fishery was the main fishery on Georges Bank. The same data that were used in this fishery definition were used to determine the catch rate coefficients for the LP equations.

The LP equations were developed to unambiguously describe the management unit catch constraints using catch rate coefficients from fisheries defined in the catch composition analysis. Several management units spanned more than one (Sub-) Division (4VWX+5 pollock) while others were totally contained within one (Sub-) Division (4X haddock). All of the fisheries spanned more than one (Sub-) Division. For example the cod fishery was found in (Sub-) Division 4Vn, 4Vs, 4W, and 4X. This fishery yielded catch from three cod management units and it was therefore necessary to develop separate cod fishery equations based on the area fished. However, it was possible to combine catch and effort data from Subdivision 4Vs and Division 4W without introducing ambiguity to the analysis. Other areas which were used were Subdivision 4Vn, Divisions 4W, 4X and Subdivision 5Ze. Catch rates were determined by fishery, area, and year for each species. Aggregations which consisted of fewer than 50 fishing sets or which had a total catch rate of less than 500 kg/hr were not included in the analysis since it was believed these were exceptional cases which did not reflect reality.

Separate LP analyses were performed using data from 1982, 1983, and the two years combined for comparative purposes. Furthermore, three objective functions were used: maximize total catch, maximize total revenue, and maximize net revenue. The objective function for maximizing total catch was given in equation (1) above. Total revenues were determined by scaling species catch rates by the respective landed prices. These prices were obtained from the Lunenburg offshore prices from the 1 December, 1982 issue of the fishery magazine Sou'wester. These prices were identical to the 15 June, 1983 and 1 May 1984 prices in the same magazine except that haddock was 2¢/lb higher on 1 May, 1984. The landed prices were converted to round weight equivalents and ¢/kg. The prices are given in Table 2. The objective function used in this case was to maximize

$$\sum_{i=1}^{m} \sum_{j=1}^{n} S_{ij} f_i P_i \qquad (2)$$

Where: P_i = the price of species i.

Two elements of variable costs were included in the calculation of net revenues: operational costs and crew costs. Estimates of operating expenses of offshore vessels greater than 100' were obtained from Carpentier et al. (1979). The mean annual per vessel operating expense, which included fuel, ice, wages (other than crew), maintenance and repair, insurance and other miscellaneous expenses, was estimated to be $432,800. Examination of DFO fisheries statistics indicate that the average offshore vessel makes approximately 22 trips per year at 12 days per trip for 264 fishing days a year. Observer data for 1982 and 1983 indicates an average of 12.26 hours fished per day for 3236.6 hours fished per year. Thus the operating costs were estimated to be $133.72 per hour. Crew costs were estimated to be 40% of the total revenues of the trip. Net revenue was estimated as 60% of total revenue less $133.72 times the number of hours fished. The objective function in this case was to maximize

$$\sum_{i=1}^{m} \sum_{j=1}^{n} f_i \, (.6 . S_{ij} . P_i - 133.72) \qquad (3)$$

Results

A total of 11 fisheries met the criteria for selection for subsequent LP analysis. The same fisheries were chosen for 1982, 1983 and the combined analyses. The catch rate coefficients used in the analyses are shown in Table 3. Fishery specific coefficients form the columns of the tables while the management unit specific coefficients form the rows. The 11 fisheries consisted of two cod directed (4Vn, 4VsW), two haddock directed (4VsW, 4X), three redfish directed (4Vn, 4VsW, 4X), two pollock directed

(4VsW, 4X), one flounder (4VsW), and one mixed cod, haddock and pollock (5Z). There were considerable variations in individual coefficient values among the three tables.

Most management units had associated with it at least one directed fishery, that is a fishery which yielded a high catch from the management unit. A notable exception was the 4X cod management unit which had a quota of 3,150 t but no cod directed fishery. A second notable situation was for the 5Z cod and 5 haddock management units. Catches for these management units could only come from the 5Z mixed fishery.

The objective function coefficients and the LP solutions for the objective of maximizing total catch are given in Table 4. Different solutions were obtained for each of the three analyses because of the different catch rate coefficients used. However, in none of the three analyses was the total available catch (129,530 t) taken. The uncaught quotas are shown in the table as surplus quotas. The 4X cod quota was not taken in any of the analyses. There were surpluses of 2,674 t, 1,680 t and 2,290 t in the 1982, 1983, and combined analyses respectively. Similarily the Subarea 5 haddock quota was never caught with 1,386 t, 5,529 t, and 2,844 t remaining. In the 1982 analysis the 5Z cod quota was also not taken. The value of the objective function (i.e. total catch) was 123,943 t, 121,520 t, and 123,595 t in each analysis respectively thus indicating little variation in total catch. However, total effort levels varied considerably with 152,082 hr, 151,481 hr, and 127,709 hr in the 1982, 1983, and combined anslyses.

The results of the LP analyses using the objective of maximizing total revenue are shown in Table 5. Both the absolute and relative values of the respective objective function coefficients under the objectives of maximizing total catch and maximizing total revenues were different. However, the optimal effort solution were identical. This resulted in identical total catches and surplus quotas under both of these objective functions.

Substantially different results were obtained under the objective of maximizing net revenues (Table 6). Several fisheries had negative objective function coefficients. None of these fisheries entered in the optimal solution since each could loose money, thus going counter to the objective of maximizing net revenues. With fewer fisheries in the optimal solution there were more surplus quotas. Again the 4X cod and 5 haddock quotas were never taken. Also, the 4VWX flounder quota was never taken. The 4VWX redfish quota was not taken in the 1983 and combined analysis. The 5Z cod quota was not taken in the 1982 analysis and the 4X haddock quota was not taken in the 1983 analysis. Substantially less total effort was expended under this objective with 97,557 hr, 72,324 hr and 86,707 hr in the 1982, 1983, and combined solutions respectively.

Observed effort distributions by fishery are shown graphically for 1982, 1983, and the 2 years combined in Figure 3. The largest single observed fishery was the 4VsW cod having 33%, 41%, and 37% of the effort in 1982, 1983 and the 2 years combined. The optimal effort distributions for the objectives of maximizing total catch and maximizing net revenue are shown in Figures 4 and 5 respectively. In the case of maximizing total catch far less effort was allocated to the 4VsW cod and the pollock fisheries and more effort to the flounder, redfish and 4X haddock fisheries than was observed. Furthermore, fewer fisheries were in the optimal solutions than observed. Under the objective of maximizing net revenue even fewer fisheries entered the optimal solutions. The amount of effort allocated to the 4VsW cod fishery was close to the observed proportions. However, there was little consistency in other comparisons among other fisheries.

Discussion

Various LP problems were analysed in an attempt to address two basic questions. The first was whether or not the individual quotas available to the mobile gear greater than 100' vessel category on the Scotian Shelf and Georges Bank could be caught without exceeding any one quota. This was analysed under the objective of maximizing total catch. Given the fishery definition developed by Sinclair (1985) and observed catch rates in these fisheries it appears that neither the 4X cod or 5 haddock quotas could be taken (Table 4). In the case of 4X cod this was mainly due to the lack of a directed fishery for this management unit and to the relatively low by-catch rates of cod in other 4X fisheries. In the case of 5 haddock the only fishery defined for this management unit was a mixed cod, haddock, and pollock fishery. Both the 5Z cod and 5 haddock quotas could not be taken from this one fishery unless the ratio of their respective catch rates was equal to the ratio of the respective quotas. Such was not the case. In the 1982 analysis the 5Z cod quota was also not taken.

The second question was with respect to how the optimal total effort levels compared to the present fleet size. Under the objective of maximizing total catch and using the 1982 catch rates the total effort was 152,082 hr, using 1983 catch rates the solution called for 151,481 hr, and in the combined analysis 127,709 hr. Using the estimated average of 3236 hrs/vessel-year quoted earlier this coresponds to 47, 47, and 40 vessels respectively. Currently there are approximately 50 vessels of this class active in the Scotia-Fundy Region. These vessels may fish areas from Georges Bank to Labrador. Thus the existing fleet may be too large for the available quotas on the Scotian Shelf, but probably not for the total catch available in all areas.

There were more fisheries observed than appear in the LP solutions. This was not unexpected since in LP solutions there cannot be more elements than constraints. In the LP problems there were 10 constraints and 11 fisheries. In the solution for maximizing total catch there were always 2 surplus quotas, and in the 1982 analysis there were 3. In the objective functions for maximizing net revenues there were

some negative elements resulting in even greater numbers of surplus quotas. Thus only a subset of the observed fisheries appeared in the LP solutions. However, with the addition of more constraints the optimal solutions may be forced to contain more fisheries.

The observed effort distributions by fishery (Figure 3) and the optimal effort distributions (Figures 4 and 5) differed substantially. When the observed effort distributions were compared to those from the objective of maximizing total catch there was a higher proportion of observed effort directed toward 4VsW cod and pollock than allocated in the LP solutions. This was likely a consequence of the fishing strategy of the fleet. In 1983 this fleet caught 77% of its cod quotas and 69% of its pollock quota while catching only 37%, 45% and 62% of its haddock, redfish and flounders quotas respectively. Since the cod and pollock quotas were the heaviest fished then it may be expected that the observed effort for these management units would be higher than the optimal effort distribution.

The inclusion of economic terms in the LP analyses did not produce a closer match between the observed and optimal effort distributions. Under the objective of maximizing total revenues exactly the same LP solutions were obtained as under the objective of maximizing total catch. Under the objective of maximizing net revenues substantially different optimal solutions were obtained. In this case there was a closer agreement between the observed and optimal effort distributions for 4VsW cod but there was little consistency in other comparisons. For example, in the 1983 and combined analyses there were no redfish fisheries in the LP solutions, but in the 1982 analysis the 4VsW redfish fishery was included in the solution and it comprised 20.1% of the total optimal effort. Similarily the 4X haddock fishery comprised 16.3% and 20.3% of the total optimal effort in the 1982 and combined analysis, but it was not included in LP solution in the 1983 analysis.

The fish prices used in the analysis are probably not indicative of the true value of the fish. The vessels are operated as part of a vertically integrated fishing, processing and marketing company. The prices are used to determine the crew wages on a share basis. Yet the companies actively direct the vessels to specific areas to fish based, presumably, on what the value of the finished product would be.

The variation among the LP solutions for the objective of maximizing net revenues indicates the sensitivity of the analysis to input parameters. The utility of LP models in formulating fisheries management regimes is limited by the often extreme and seemingly unrealistic nature of the solutions (Shepherd and Garrod 1982). The use of additional constraints to simulate other important factors may ameliorate this problem but this would require substantial increases in parameter formulation and estimation. However, the method provides a framework for analysing some questions regarding allocations among fleet components and the possibilities of fleet components actually taking an array of catch quotas.

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Discussion Period

O'Boyle: The offshore allocations, were these the actual enterprise allocations?

Sinclair: They were the quotas for the fleet as a whole not the enterprise allocations?

Murawski: Inasmuch as the optimal allocation differs so much from the current pattern it appears then that industry is operating irrationally. Is this surprising?

Sinclair: With some economic considerations we might find that there is not such a discrepancy. As I pointed out, the prices I used may be inappropriate. Also the fleet traditionally does not catch each quota. Flatfish and redfish quotas were not caught in 1982. This would have some influence on the apparent difference between the optimal and observed effort distributions. Certainly in the real world situation the fisheries are much less predictable than in the equations, so there are several areas where this discrepancy could arise.

McKone: Just a couple of observations: one is that the companies do not necessarily look to profitability on each trip. They look to an annual profit. For example the companies might be willing to take a loss on the redfish fishery to keep the plants open, knowing that they make profit over all. Secondly, in averaging the vessel costs you may have distorted the picture. Steaming out to fish redfish on the edge of the shelf costs more than fishing other species closer in.

Sinclair: I considered that, but a lot of these fisheries are on offshore banks. Also the plants are widely distributed throughout the area, so how far the vessel steams depends on where it ended up on the previous trip rather than were it fishes. If I were considering a wider scale, say including the Grand Banks, there could be gains to be made by considering trip costs as available. But on the Scotian Shelf I am not sure it matters.

McKone: I think the largest cost is the cost of fuel. So distance steamed is very significant. Hence the demand for redfish in the Gulf where they are close, even though they are available in 2J 3K. So one really needs to consider these factors.

Table 1. Distribution of groundfish management units with respect to NAFO areas and the associated quotas for mobile gear greater than 100' in 1983.

Management Unit	4Vn	4Vs	4W	4X	5	Quota (t)
4Vn Cod (D-A)	X					5,080
4Vn Cod (M-D)	X					800
4VsW Cod		X	X			33,550
4X Cod				X		3,150
5Z Cod					X	11,250
4VW Haddock	X	X	X			11,850
4X Haddock				X		7,050
5Z Haddock					X	7,300
4VWX Redfish	X	X	X	X		21,500
4VWX+5 Pollock	X	X	X	X	X	19,650
4VWX Flounder	X	X	X	X		8,350
				TOTAL:		129,530

Table 2. Landed prices (¢/kg) converted to round weight equivalents. From Sou'wester, December 1, 1982.

Cod (market)	46.2
Haddock (large)	58.7
Redfish	24.8
Pollock (large)	27.1
Flounder (round)	27.5

Table 3. Catch rate coefficients used in the 1982, 1983, and combined LP analyses.

	Fishery										
Management Unit	Cod		Haddock		Redfish			Pollock		Flounder	Mixed
	4Vn	4VsW	4VsW	4X	4Vn	4VsW	4X	4VsW	4X	4VsW	5Z
1982											
Cod 4Vn	1415	0	0	0	16	0	0	0	0	0	0
4VsW	0	1308	90	0	0	43	0	30	0	155	0
4X	0	0	0	30	0	0	12	0	25	0	0
5Z	0	0	0	0	0	0	0	0	0	0	379
Haddock 4VW	1	49	1163	0	0	11	0	29	0	6	0
4X	0	0	0	444	0	0	18	0	59	0	0
5	0	0	0	0	0	0	0	0	0	0	213
Redfish	2	12	0	42	332	1046	665	21	110	8	0
Pollock	38	47	42	27	0	84	13	1005	1682	1	634
Flounder	1	60	3	4	3	22	3	1	1	599	0
1983											
Cod 4Vn	802	0	0	0	35	0	0	0	0	0	0
4VsW	0	1207	51	0	0	18	0	93	0	126	0
4X	0	0	0	46	0	0	8	0	4	0	0
5Z	0	0	0	0	0	0	0	0	0	0	648
Haddock 4VW	11	43	695	0	1	2	0	30	0	2	0
4X	0	0	0	245	0	0	16	0	35	0	0
5	0	0	0	0	0	0	0	0	0	0	102
Redfish	5	8	1	15	683	774	702	30	3	6	0
Pollock	8	48	21	78	1	6	56	1412	2202	3	248
Flounder	20	47	5	7	3	11	1	3	0	265	0
Combined											
Cod 4Vn	1015	0	0	0	34	0	0	0	0	0	0
4VsW	0	1248	75	0	0	24	0	72	0	142	0
4X	0	0	0	34	0	0	9	0	20	0	0
5Z	0	0	0	0	0	0	0	0	0	0	457
Haddock 4VW	8	45	983	0	1	4	0	30	0	4	0
4X	0	0	0	393	0	0	17	0	54	0	0
5	0	0	0	0	0	0	0	0	0	0	181
Redfish	4	10	1	35	657	835	690	27	85	7	0
Pollock	18	47	34	40	1	24	42	1279	1800	2	523
Flounder	13	52	4	5	3	14	2	3	1	456	0

Table 4. Objective functions, optimal effort distributions, and surplus quotas associated with the objective of maximizing total catch in three LP analyses.

		1982		1983		COMBINED	
Fishery		Objective Function	Optimal Effort	Objective Function	Optimal Effort	Objective Function	Optimal Effort
Cod	4Vn	1.457	2,893	.846	6,334	1.058	5,005
	4VsW	1.476	23,706	1.353	23,939	1.402	24,530
Haddock	4VsW	1.298	9,131	.733	15.089	1.097	10,830
	4X	.547	15,878	.391	26,869	.507	16,413
Redfish	4Vn	.351	61,609	.723	0	.696	0
	4VsW	1.206	0	.811	0	.901	0
	4X	.711	0	.783	29,191	.760	29,553
Pollock	4VsW	1.086	0	1.568	7,097	1.411	0
	4X	1.877	0	2.244	0	1,960	1,798
Flounder	4VsW	.769	11,100	.402	25,601	.611	14,963
Mixed	5Z	1.226	27,764	.998	17,361	1.161	24,617
Objective Total (t)		123,943		121,520		123,595	
Effort Total (hr)			152,082		151,481		127,709
Surplus Quotas		4X Cod	2,674	4X Cod	1,680	4X Cod	2,290
		5Z Cod	727	5 Haddock	5,529	5 Haddock	2,844
		5 Haddock	1,386				

Table 5. Objective functions, optimal effort distributions, and surplus quotas associated with the objective of maximizing total revenue in three LP analyses.

		1982		1983		COMBINED	
Fishery		Objective Function $/hr	Optimal Effort hr	Objective Function $/hr	Optimal Effort hr	Objective Function $/hr	Optimal Effort hr
Cod	4Vn	665	2,983	386	6,334	483	5,005
	4VsW	665	23,706	661	23,939	633	24,530
Haddock	4VsW	737	9,131	439	15,089	623	10,830
	4X	293	15,878	192	26,869	267	16,413
Redfish	4Vn	90	61,609	187	0	180	0
	4VsW	314	0	206	0	230	0
	4X	185	0	202	29,191	197	29,553
Pollock	4VsW	308	0	451	7,097	404	0
	4X	529	0	619	0	549	1,798
Flounder	4VsW	242	11,100	135	25,601	196	14,963
Mixed	5Z	472	27,764	426	17,361	459	24,617
Objective Total ($,000)		50,439		48,800		50,095	
Effort Total (hr)			152,081		151,481		127,709
Surplus Quotas (t)		4X Cod	2,674	4X Cod	1,680	4X Cod	2,290
		5Z Cod	727	5 Haddock	5,529	5 Haddock	2,844
		5 Haddock	1,386				

Table 6. Objective functions, optimal effort distributions, and surplus quotas associated with with ojbective of maximizing net revenus in three LP analyses.

Fishery		1982 Objective Function $/hr	1982 Optimal Effort hr	1983 Objective Function $/hr	1983 Optimal Effort hr	COMBINED Objective Function $/hr	COMBINED Optimal Effort hr
Cod	4Vn	266	3,590	98	6,334	156	5,005
	4VsW	265	24,387	233	27,151	246	26,233
Haddock	4VsW	308	8,973	130	15,270	240	10,813
	4X	42	15,878	-19	0	27	17,605
Redfish	4Vn	-79	0	-22	0	-26	0
	4VsW	55	19,630	-10	0	5	0
	4X	-23	0	-12	0	-16	0
Pollock	4VsW	51	0	137	0	109	0
	4X	184	0	238	6,208	196	2,434
Flounder	4VsW	12	0	-53	0	-16	0
Mixed	5Z	149	25,099	122	17,361	142	24,617
Objective Total ($,000)		15,689		12,516		14,258	
Effort Total (hr)			97,557		72,324		86,707
Surplus Quotas (t)	4X Cod		2,674	4X Cod	3,125	4X Cod	2,503
	5Z Cod		1,737	4X Haddock	6,833	5 Haddock	2,844
	5 Haddock		1,954	5 Haddock	5,529	4VWX Redfish	20,384
	4VWX Flounder		6,361	4VWX Redfish	21,217	4VWX	20,384
				4VWX Flounder	6,871		

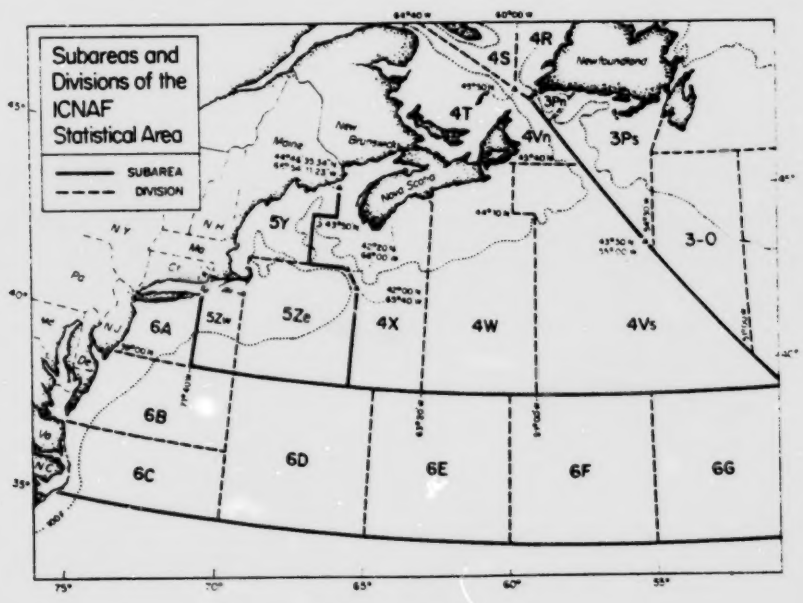

Figure 1. NAFO Divisions and Subdivisions in the study area.

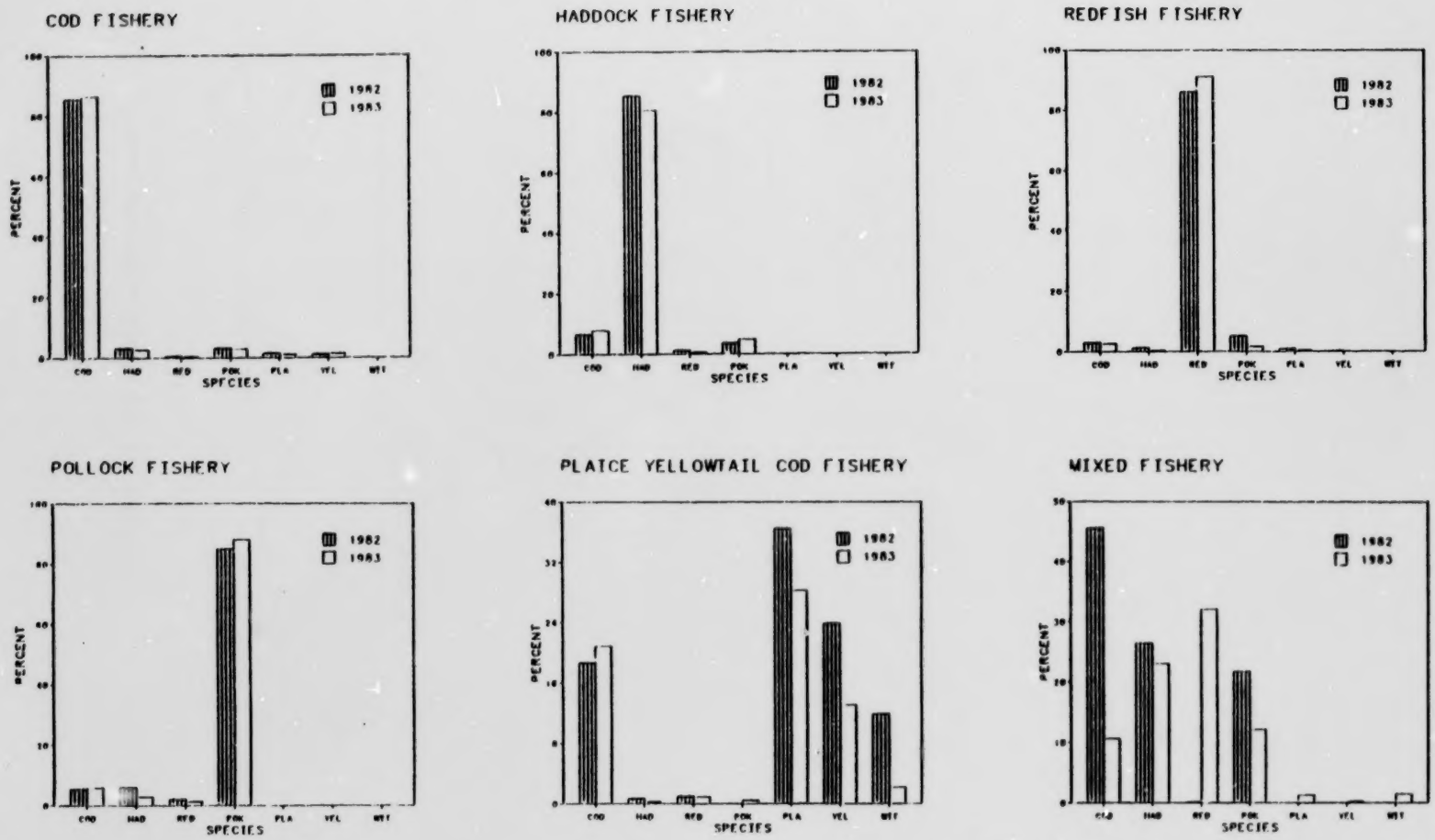

Figure 2. Species compositions of the fisheries identified by cluster analysis of 1982 and 1983 observer data from Canadian TC4-6 otter trawlers.

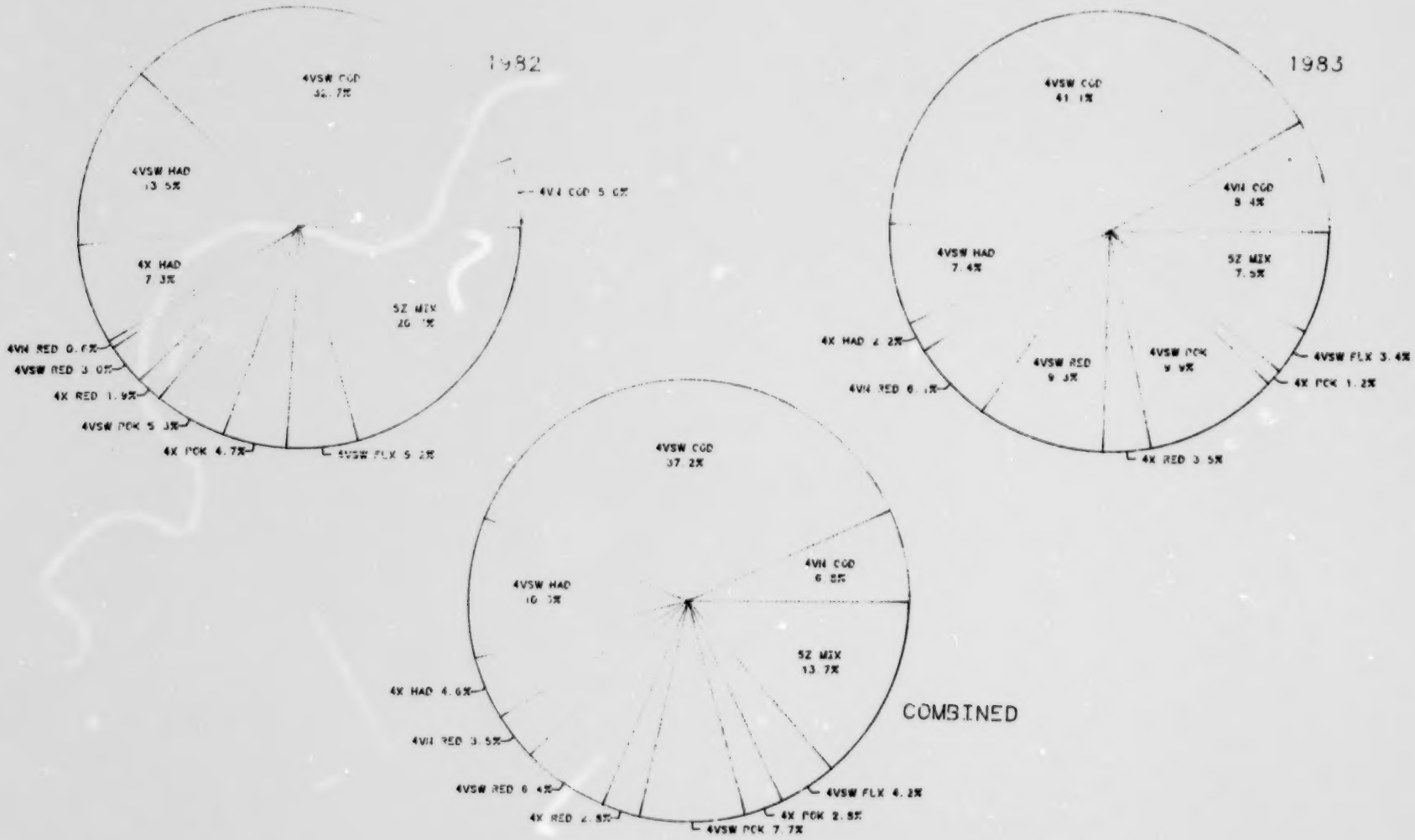

Figure 3. Observed effort distribution by fishery.

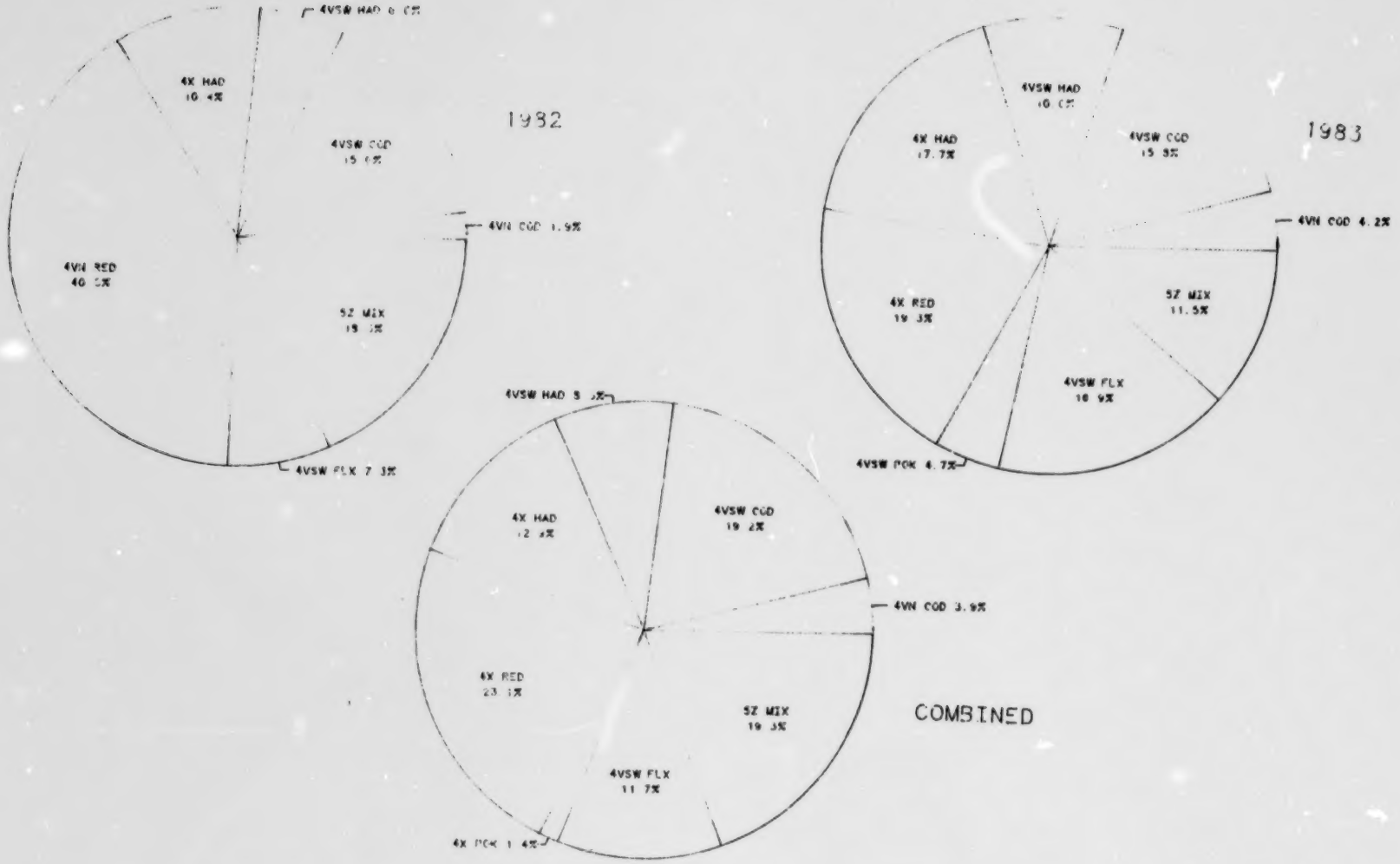

Figure 4. Optimal effort distribution by fishery resulting from the objective of maximizing total catch and total revenue.

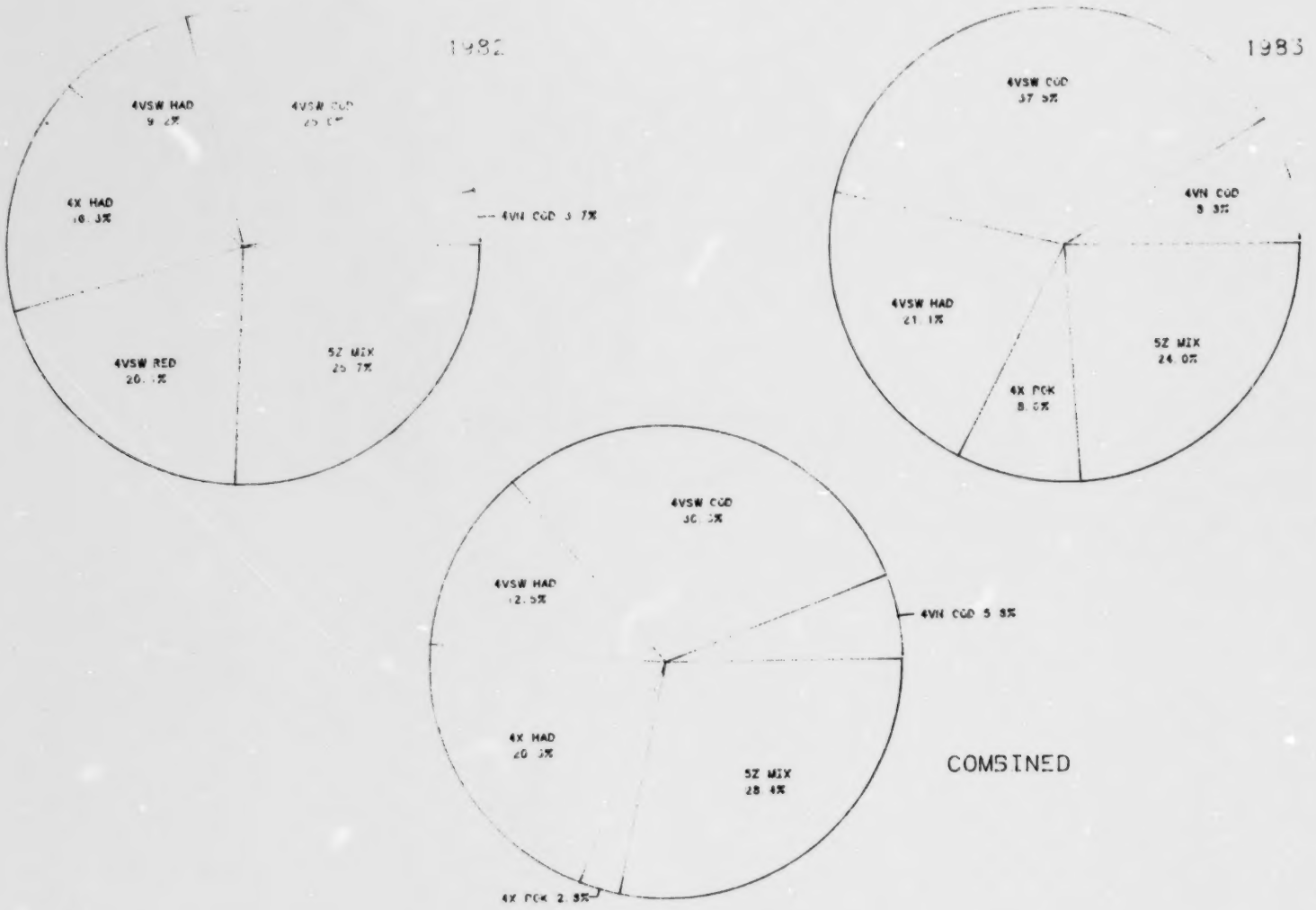

Figure 5. Optimal effort distribution by fishery resulting from the objective of maximizing net revenues.

Implications of Multispecies Principles for Canada's East Coast Fisheries Management Advisory Process

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Since the foundation work in fisheries science by Beverton and Holt in 1957, fisheries management organizations in the North Atlantic have grown, structured around the single species approach. Although multispecies theory has progressed, it has rarely been used in practical management. Before it can, substantial change to the existing structure should occur. First, although scientific advice is generally well reviewed, the management advisory process is not. A similar structure for the latter as with the scientific advise is advocated. Once this has occurred then implimentation of multispecies approaches can be attempted. Since the theory underlying biological interactions is weak and still developing it is recommended that first attempts at multispecies management consider technological interactions only. The management structure would have to evolve as our multispecies theory progresses. The present taxon group approach should over the longer term give way to an ecosystem approach. Whatever changes are implemented, they must occur at all levels of the advisory process and not be restricted to any one level.

Introduction

When Beverton and Holt (1957) introduced their ideas on the dynamics of exploited fish populations, the fisheries of many nations were in desperate need of management to reverse the declining trends in resource abundance. Two elements needed to be considered. First was the definition of the management objectives, which depended in large part on the models presented by Beverton and Holt (1957). Although these two authors considered a wide spectrum of population models, including multispecies and economic, the existing data sets only allowed formulation of objectives on a single species basis.

The second element of management was consideration of the administrative structures needed to achieve the management objectives. On both sides of the Atlantic, existing organizations (ICES* in the east and ICNAF in the west) took the Beverton and Holt single species models and fashioned management advisory processes around them. These structures have continued in a similar form to the present within organizations such as NAFO and CAFSAC in the Northwest and ICES in the Northeast Atlantic. Consequently the existing Canadian east coast management structure is a result of over 30 years development of a single species approach.

Some pursuit of objectives based on multispecies principles has occurred. During 1972-76, ICNAF was the forum for much dialogue on multispecies models and even put some of this theory into practice, using what is referred to as a "Two Tier Catch Quota System". ICES work (Anon., 1984), on the other hand, has not yet advanced beyond the stage of model development.

For the most part then the management objectives and administrations responsible for the North Atlantic fisheries have been based on a single species philosophy. Although multispecies ideas have been considered, little practical management using them has occurred.

There is a growing realization that management organizations should manage fisheries and not just fish. This requires restatement of management objectives in terms of biology, economics, and sociology. Not only must biological interactions be considered but so too technological ones. A change in the objectives may require a change in the administrative structure needed to achieve these objectives.

This paper considers the evolution of the management objectives since the 1950s and examines the management systems in the North Atlantic in the context of these objectives. Suggestions are made on how best to change the structure on Canada's east coast to meet the new demands being made on the system.

The Objectives of Fisheries Management on Canada's East Coast

The most important feature of any management scheme are its objectives. In the fisheries of the Northwest Atlantic, these have changed considerably over time.

Needler (1979) recently summarized the current policy for resource management as:

> "to obtain from exploitation of the resource the greatest possible benefits to society as a whole, and more particularly, to assure the economic welfare of fishermen and fishing communities, including the fish processing industry and the fish trade".

This is a long way from what it was prior to 1950.

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* see Glossary for meaning of abbreviations.

At that time most coastal resources were in international waters and access to them was unlimited. Fisheries management per se did not exist. With the instigation of ICNAF in 1949, the first attempts at fisheries maangement were made. Initially, biology formed the basis of management objective formulation, chief among these being conservation. As was stated above, the administrative structures were established around the biological models of Beverton and Holt (1957). Gordon (1953; 1954) introduced the field of fisheries economics but it wasn't until the late 1970s that ICNAF considered economics in its management objectives, in this case, attainment of the $F_{0.1}$ exploitation rate. Nevertheless, the biological models introduced in the 1950s persisted, except now employed in a different capacity. The management structures remained the same.

With the extension of Canada's jurisdiction to 200 miles came the realization that proper resource husbandry would require more explicit consideration of economic and social issues. Indeed the emphasis in objectives shifted from management of fish to management of fisheries (Kirby, 1982).

This historical development has been paralleled in most areas of the North Atlantic. Below is reviewed the management systems of Canada, USA, and Europe to see how each has handled the new set of objectives.

The Structure of Existing Management Systems in the North Atlantic

Canada

Management is pursued separately for groundfish, pelagic fish, marine mammals, freshwater species, scallops, shrimps, crabs, and other invertebrates. Within each of these, management plans are formulated on a yearly basis. CAFSAC is requested to evaluate the size of the resources annually. This information is passed on to the ADGC for its initial perusal (Figure 1). From here, the information is given to species specific Advisory Committees, such as the AGAC, which have the responsibility for formulating annual management plans. In fact, AGAC instructs regional advisory committees, consisting of government and industry representatives, to develop regional plans. AGAC rolls these regional plans up into one plan for the east coast. This is then presented to the DFO deputy minister's office. From here it goes to the FPAFC, and DFO Minister's office and finally, if necessary, to Cabinet. Upon Cabinet approval, the plan becomes public. The overall structure is along taxon lines with little interaction among the groups handling different taxa.

USA

Since 1977, fisheries management in US coastal waters has been under the direction of regional Management Councils. In the North Atlantic, the main one is the NEFMC. This body has the responsibility of formulating one management plan for each taxonomic group, as in Canada. However the plan is not rewritten annually but is supposed to endure over a longer period of time. The Council has its own staff of biologists and economists which evaluate different proposals made by industry. NMFS only enters the process upon request.

The key difference between this and the Canadian system is that it is A) highly regionalized and B) has a higher degree of industry participation.

Europe

The European management structure is similar to that of Canada (Figure 2). ICES, like CAFSAC, provides biological advice on a yearly basis through the ACFM to the EEC Commission (Lee, 1973; Frost, 1984). The latter has a STC which is supposed to introduce economic principles into the advisory process. The management plans are then passed into the political arena (Council of Ministers of Fisheries) for final approval.

Synopsis

All the systems examined above have strong and weak points. Canada's system benefits from a well organized scientific advisory process as well as limited foreign concerns which can introduce large political influences into the system. The European system, like Canada, has a strong scientific base but has a large element of international interaction which can make concensus and goal setting difficult (Frost, 1984). The US system's main strength is the degree of involvement by industry. This ensures acceptance of management plans. As well, scientific input, although not as organized as in Canada and Europe, comes from a variety of sources. Nevertheless, its major weakness is that in effect, the patient is being asked to regulate its own pain. This has resulted in a number of ineffectual regulations being proposed.

All these organizations have in common the strong biological input into the management process. Both Canada and Europe have extensive peer review processes to ensure quality of biological advice. The same cannot be said regarding the integration of this information with economic and social models, a prerequisite of multispecies management. In all three systems, once the biology has been stated, the decision making process becomes hard to identify. This lack of explicitness is to a certain extent encouraged by the industry (allows room for lobbying) and introduces uncertainty into the decision making process. Experience has shown (Frost, 1984) that this leads to short-term increases in effort along with long-term investment restraint (get what you can now; it might not be there tomorrow). This lack has often led to the use of biologists to arbitrate on allocational issues.

It is evident from the above discussion that what each organization lacks is a structured management committee which can provide for the integration of biological, economic, and social data and models. Edwards (1981) has termed this the "Excluded Middle". What is required in the organization is a body of experts who can synthesize the biological, economic, and sociological information. Individuals in this organization would have to have a quite eclectic background in order to communicate intelligently with the various players in the system.

Although there are signs in each of the three systems examined of such a group evolving, no structure has yet been established. Many problems that we have in the existing systems can be traced to this missing part -- certainly with regard to allocational issues (Bockstael, 1980). These problems will no doubt persist or become exacerbated in a system oriented towards multispecies management. It certainly cannot be said unequivically that the problems in the effective management in each country are due to the use of single species models, and thus whether or not these could be alleviated by movement to a multispecies approach. It seems evident that changes can be made to the existing organization to improve management performance. At the very least, these would have to predate the implementation of multispecies management on a large scale. An idea of the required changes, in the context of the Canadian east coast, is discussed in the next section.

The Changes Implicated by Multispecies Management

Changes to the Present System

At present CAFSAC provides not only information on historical stock trends to managers, but also forecasts on yield expectations. As was stated earlier, the problems arise in the interpretation and use of this data in the next level of the management hierarchy. With a move towards multispecies management, the objectives and strategies will become a mixture of biological, economics, and social constraints, making interpretation all the more difficult.

It is proposed here that CAFSAC retain its function as the purveyor of scientific knowledge. To this mandate should be added expertize in economics. CAFSAC would be a forum for the review of fisheries models, as is now the case but would refrain from providing explicit objectives, such as $F_{0.1}$, to managers. The use of economic theory is within the already stated terms of reference of this committee (Table 1).

It is proposed that the existing RMAC structure be reorganized along the same lines as CAFSAC. Procedures developed and approved in CAFSAC would be tabled for use in the RMACs. These bodies would consist of managers, biologists, economists, industry representatives, etc., and would be responsible for devising long and short-term management strategies in a peer review forum. Industry would benefit from government expertize while managers would benefit from the knowledge of what management scenarios will most likely meet industry compliance.

Once the existing structure has been changed, the implementation of multispecies models would be facilitated. Indeed, significant work by CAFSAC on multispecies models without changes to the management structure may be counter-productive.

Implementation of Multispecies Management

Multispecies interactions can be categorized as being biological (predation, competition, etc.) or technological (by-catch, gear conflict, etc.).

The benefits of considering biological interaction could be substantial. However, the level of knowledge of these interactions is low. Past attempts to incorporate them (Lett and Doubleday, 1976) into the management advisory process have not met with success. It is proposed that while research continues on these interactions, changes to the management structure to consider them need not be made now. Nevertheless, any changes that do occur now should take into account the eventuality that biological interactions will be considered in the advisory process.

Consideration of technological interactions can occur presently. It is thus proposed that the management structure be established to accommodate them.

With the above comments in mind, a chronology of change to the present structure could be as follows:

1) The present CAFSAC and RMAC be retained with more structure and responsibility given to the latter.

2) Areas and/or fisheries with high technological interaction be identified (perhaps by CAFSAC (MEES)) with the establishment of working groups attached to the existing subcommittees and advisory committees. These might be best organized along geographic (i.e. 4X) rather than taxon (i.e. groundfish) lines, dependent on the degree of interaction among fish and invertebrate fisheries.

3) Identification of ecosystems that are meaningful for fisheries management be pursued within CAFSAC. Once these are established, both CAFSAC and RMACs could be rearranged to correspond to them. The total number of such groups would have to remain small so as not to compromise the peer review process.

4) Discussion on technological interactions would be pursued as before in these ecosystem groups. As well, biological concerns would be introduced.

The guiding principles of these proposed changes are A) there must be symetry between the model builders (CAFSAC) and the model users

(RMACs); B) change must occur gradually to ensure benefit to the management process; C) the structure must not compromise the peer review process; D) it must be based on the realities of knowledge of the system; and E) it must be flexible and adaptable to change.

The above schedule may take a long time to complete and can thus be viewed as a direction to move rather than as a detailed plan for change. However, it is felt that change is needed and the above offers a starting point for discussion.

Summary

A review of three management systems points out that before effective incorporation of multispecies principles can occur, some changes need to be made to the existing structure. These relate to the division of labour between the scientists and the managers. Once these are in place, a multispecies approach can be considered.

It is realized that the single species approach places constraints upon effective resource management. Consequently, the move toward multispecies management is viewed as desirable. However, a rapid change in the structure would be both unnecessary and resisted by the existing organization. Change must be based on practicality as well as theory. In the short term, technological interactions could be addressed through working groups attached to status quo committees. As the information base grows, these would also consider biological interactions and eventually attain committee status. This however is looked on as a long-term objective.

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Acronyms

ACFM	Advisory Committee on Fisheries Management
ADGC	Atlantic Director Generals Committee
AFS	Atlantic Fisheries Service
AGAC	Atlantic Groundfish Advisory Committee
CAFSAC	Canadian Atlantic Fisheries Scientific Advisory Committee
EEC	European Economic Community
FPAFC	Federal/Provincial Atlantic Finfish Committee
ICES	International Council for the Exploration of the Sea
ICNAF	International Commission for the Northwest Atlantic Fisheries
MEES	Marine Environment and Ecosystem Subcommittee
NAFO	Northwest Atlantic Fisheries Organization
NEFMC	New England Fisheries Management Council
NMFS	National Marine Fisheries Service
RMAC	Resource Management Advisory Committee
STC	Scientific and Technical Committee

Table 1. Terms of Reference for Canadian Atlantic Fisheries Scientific Advisory Committee (CAFSAC).

CAFSAC is responsible for providing scientific advice to the Atlantic Directors-General Committee on the management, including the full range of conservation measures taking into account economic objectives, of all stocks of interest or potential interest to Atlantic coast fishermen. Resource management advice will be provided in accordance with specific fisheries management objectives and strategies and will normally be published as a matter of routine.

CAFSAC is to serve as the Atlantic focus for development of fisheries resource management science through program development and scientific interchange. As a forum for the advancement of fisheries management science, CAFSAC shall organize workshops and symposia on specific problem areas.

In cooperation with AFS Headquarters, CAFSAC will serve as a forum for development of proposals for cooperative research and scientific monitoring of foreign fishing in the NW Atlantic. CAFSAC shall advise on the needs for scientific monitoring data, both in quality and quanitity required for effective monitoring of fishing activity.

CAFSAC will endeavor to ensure liaison with other committees or subcommittees established by the Atlantic Directors-General Committee. Such liaison will include mutual referral and it will also include joint meetings with other fora of consultation so as to ensure advice arising from the various lines is as compatible as possible and is consistent with long-term objectives for Atlantic fisheries.

In relation to the above functions, CAFSAC will review research priorities and performance in the Atlantic regions and shall advise the Atlantic Fisheries Research Directors when changes in priorities, program objectives, or resources appear warranted.

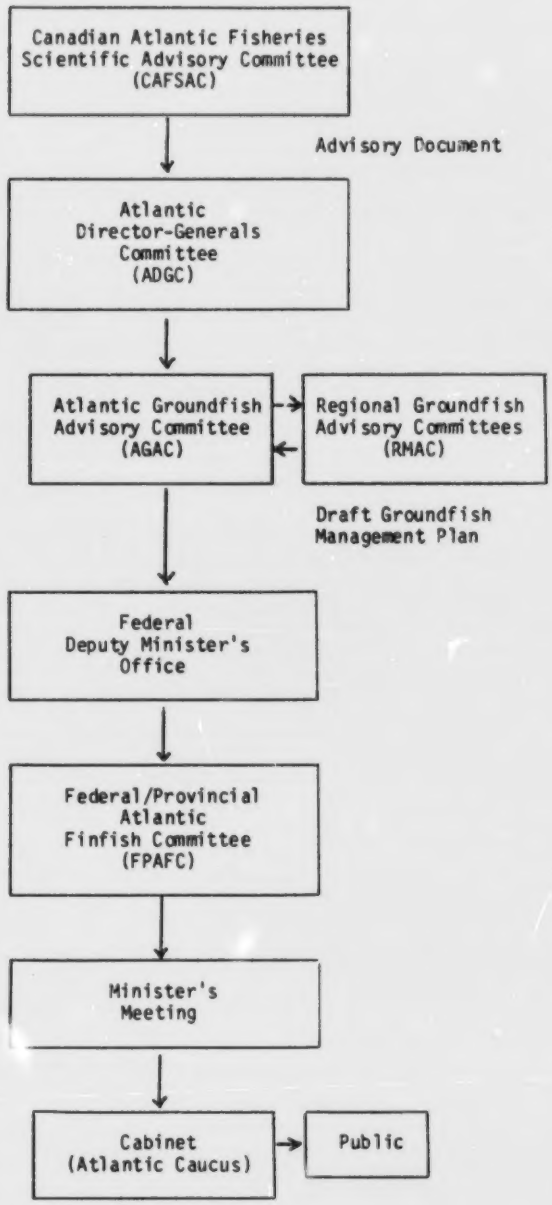

Figure 1. Flow of fisheries management advice in Canada's east coast groundfish fisheries.

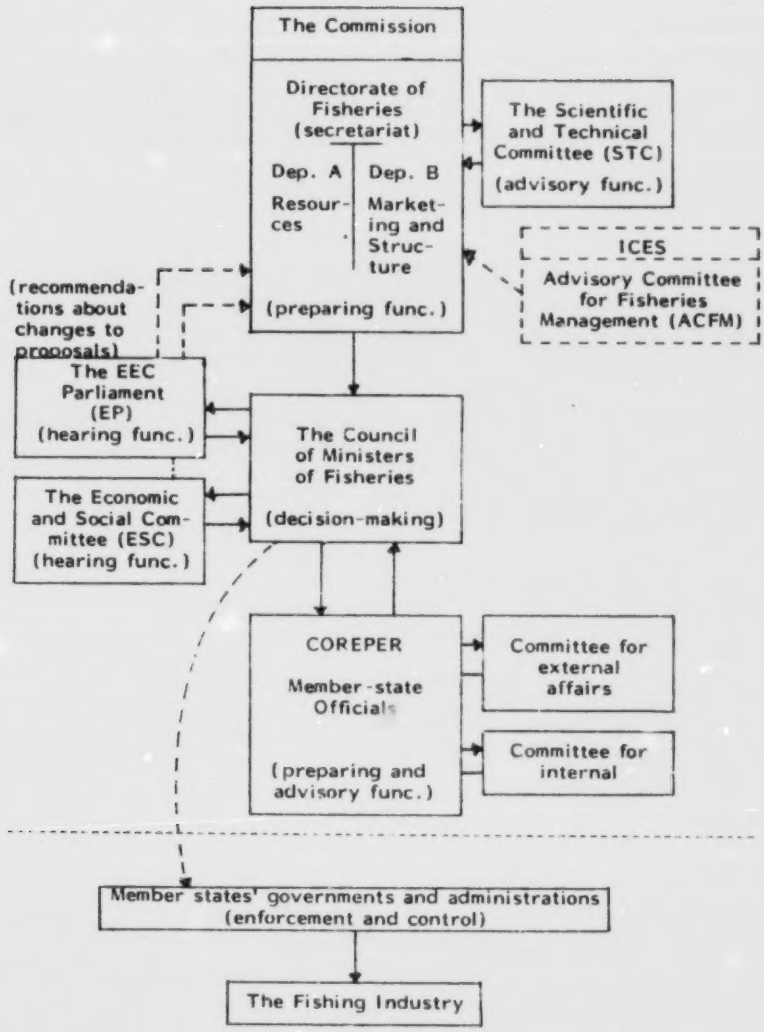

Figure 2. Fisheries Administration within the EEC (from Frost, 1984).

SESSION III:

METHODS FOR IDENTIFICATION OF MULTISPECIES SYSTEMS

Causal Analysis of Benthic Community Structure

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Schwinghamer, P. 1985. Causal Analysis of Benthic Community Structure. p. 111. In: R. Mahon [ed.] Towards the Inclusion of Fishery Interactions in Management Advice. Can. Tech. Rep. Fish. Aquat. Sci. No. 1347.

In the analysis of structural data such as biomass spectra of marine benthic communities, we try to formulate hypotheses that describe the underlying dynamics. Although not widely used in ecology, a group of quasi-statistical methods known in the social sciences as causal analysis provides an approach to generating hypotheses from non-experimental data that is ideally suited to ecological applications. Heise (1975) gives an introduction to the philosophy and some methods of causal analysis including path analysis of linear systems.

Schwinghamer (1983) examined cross-sectional and time-series data on benthic biomass spectra at a number of stations in the Bay of Fundy using causal analysis methods. The author examined numerous causal structures for two data sets and, using path analysis and prior biological knowledge, constructed plausible models of cause and effect relationships within two sets. Fitting such models to the data was a compromise, but it was assumed that if the variables and relationships were correctly identified and measured, then as part of the model more closely fit "reality", the fit of all other parts would improve insofar as the parts were interactive. Path analysis indicated that the hypothesized causal models led to correlation matrices that corresponded closely to those inherent in the data sets. It was the intent of the work to demonstrate a method of formulating ecological hypotheses from complex, multivariate field data. The identification of possible causal relationships implied mechanisms of interaction among variables that should then be investigated in more finely focussed studies.

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Discussion Period

Murawski: It appears that there could be a number of variables in the model which describe the same thing, and I wonder how judicious one must be in using only those variables which describe one particular phenomenon.

Schwinghamer: Very, it's necessary to use your head rather than a cookbook approach and to have some insight in the beginning. Here carbon and nitrogen are related and only one is used.

Myers: One can also use latent variables in the analysis, and these are unobserved. Then you include all the different variables which are assumed to be measuring the same thing, and test whether this is infact the case.

Neilson: How does the simple summation of the least squares correlation coefficients in this case better enable you to assess causality rather than simply examining the correlation matrix? After all, you are still choosing the path based on your own experience and bias.

Schwinghamer: In the method of path analysis, the whole model structure can be affected by the direction and origin of causation hypothesized for a bivariate correlation. Thus, each causal hypothesis uses all the information in the data set which is not possible otherwise.

Neilson: How do you treat degrees of freedom in determining the significance of the relationships, is that well worked out?

Schwinghamer: Yes, it is well worked out - similar to testing significance of regression coefficient where degrees of freedom are taken into account.

O'Boyle: It appears to me that with all the relationships in the model, some of them are likely to be nonlinear, is that a problem?

Schwinghamer: It did not appear to be a major problem in my data set. Some apparently nonlinear relationships can be dealt with by transformation, and some can be resolved by a multivariate approach.

Myers: Path analysis is restricted to linear relationships or those which can be transformed to linear.

Silvert: This is a chronic problem in using statistical techniques. I would like to keep in mind that Peter's paper involves the formulation of hypotheses and that sometimes it is useful to use a linear technique to look at hypotheses about nonlinear systems. However, the next step before accepting the model is to look at the biology. Peter did emphasize that it takes a lot of biological insight to use these approaches.

O'Boyle: It would be an interesting exercise to take the Andersen and Ursin model of the North Sea, add noise to it and then have it analysed according to this approach.

The Use of Cluster Analysis in Identification and Description of Multispecies Systems

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Gabriel, W.L. and S.A. Murawski. The Use of Cluster Analysis in Identification and Description of Multispecies Systems. p. 112-117. In: R. Mahon [ed.] Towards the Inclusion of Fishery Interactions in Management Advice. Can. Tech. Rep. Fish. Aquat. Sci. No. 1347.

Although several techniques exist to explore and simplify patterns in large data matrices, some attributes of cluster analysis make it an appropriate tool to address specific aspects of identification and description of multispecies fishery systems. In early phases of model development, hierarchical cluster analysis is an efficient method of examining effects of several levels of resolution or aggregation simultaneously, but the analysis cannot a priori distinguish environment, system, and mechanism. Bottom trawl survey data and multispecies landings information have been clustered to define ecological (e.g., assemblage production units) or operational groupings for fishery management purposes. The technique may be applied to other problems in fisheries ecology and management as well. Criticism of arbitrary interpretation of group definition can be minimized in several ways, including clear definition of model objectives, evaluation in terms of extrinsic patterns and simultaneous application of ordination techniques.

Introduction

Although several techniques exist to explore and simplify patterns in large data matrices, some attributes of cluster analysis make it a more appropriate tool to address specific aspects of identification and description of multispecies fishery systems than others. The objectives of this paper are to identify those aspects of system identification and cluster analysis that are compatible, in light of some uniques characteristics of cluster analysis; and provide examples of application of the analysis to descriptions of multispecies fishery systems.

System Identification

Especially for the development of theoretical, quantitative system models, successful system identification depends on explicit objectives: what is the purpose of the model, what will it do, what will the end product be and what is the question to be answered? Objectives will raise a specific question. The first problem in system identification is identifying what subsystem out of the entire economic/ecosystem the question relates to. What is environment and what is system? What is system and what is mechanism? Objectives will also include criteria for an acceptable answer. How well must the answer be resolved in time and space? Must it match particular theories or observations, and if so, how closely? What levels of aggregation can or must be accommodated?

Properties of Cluster Analysis

Cluster analysis involves the formation of groups of entities with similar attributes (e.g., groups of sites with similar species compositions). Although several forms of the technique exist, discussion here will be limited to the hierarchical agglomerative form, the most familiar form which gives rise to most dendrograms. (In hierarchical forms, relationships between constituent entities can be measured; while they are not individually defined in non-hierarchical forms. In agglomerative forms, individual entities with similar characteristics are joined to form larger groups; while in non-agglomerative (divisive) ones, the entire group of entities is subdivided into successively smaller groups.) A dissimilarity (distance) index is calculated for each pair of entities, based on similarity of attributes between each, analogous to a correlation coefficient. Similar entities are then grouped together (fused), based on an algebraic rule chosen by the investigator. In turn, small groups are joined together to form larger ones, based on the same rule. The dendrogram ("tree diagram") shows the dissimilarity level at which entities and groups were fused. The variety of distance indices and fusion strategies is too wide to discuss here; however, each index has slightly different characteristics, which should be compatible with study objectives. The basic methodology is reviewed in several texts (e.g., Sneath and Sokal 1973, Clifford and Stephenson 1975, Boesch 1977, Williams and Lance 1977).

Groups formed from cluster analysis are hierarchical; after the analysis, constituent small groups with relatively homogeneous composition can be aggregated into large groups, each with relatively heterogeneous composition. Thus, several levels of resolution or aggregation are included at once in the analysis. The hierarchical nature allows easy transition from large- to fine-scale group definition. If a chosen degree of resolution turns out to be too high or too low to address a problem, interpretation of the same analysis can be refocused subjectively at a different level of group size and within-group homogeneity. In early phases of objective development, this is an efficient method of examing effects of several levels of resolution simultaneously.

Flexibility of interpretation allows extraction of more information from the analyses than if rigid rules were required. Dissimilarity levels at which groups form may vary from group to group, as a group characteristic. Potential information about characteristic within-group heterogeneity is not discarded as a consequence.

Cluster analysis, as a classification procedure, is inappropriate for characterizing systems organized along a gradient. Distinct boundaries are formed between groups of entities, because each observation can belong to only one group. What may be a gradient in, say, species composition over area, may be split up in the analysis process, leading to boundaries with no immediate interpretation.

Fishery-Related Examples

The assemblage production unit (APU) has been proposed as the ecological basis for multispecies management, as a group of trophically coupled, resident species within a geographically limited natural production system (Tyler et al. 1981). Cluster analysis of survey data was cited as a basis for assemblage mapping, the initial step before evaluating trophic linkages.

Cluster analyses of research survey data, in most cases for initial descriptions of assemblage production units, have been undertaken for the Oregon coast (Gabriel and Tyler 1980), the Gulf of Alaska continental shelf (Gabriel and Tyler 1981, Gabriel 1982), Georges Bank (Overholtz and Tyler 1985), the Eastern Bering Sea (Walters and McPhail 1982), and the Scotian Shelf and Bay of Fundy summarized by Mahon (1985). The large scale studies of Pacific demersal assemblages showed depth and circulation patterns as geographic boundaries, while geographic boundaries to assemblage distributions on the Oregon coast, Georges Bank, the Eastern Bering Sea, Scotian Shelf and Bay of Fundy appeared primarily structured by depth. Where time series were available (Georges Bank, Eastern Bering Sea, Scotian Shelf and Bay of Fundy, and preliminary results from the Oregon coast), certain boundaries reoccurred over time. For Georges Bank, three phases of species composition over fifteen years were also identified using cluster analysis (Overholtz and Tyler 1985). In terms of system identification, these types of analyses have enabled the initial definition of ecological systems in time and space. They have also provided indications of the lowest levels of resolution or aggregation that are repeatable over time.

From the techological perspecitive, mixed species fisheries have been based on cluster analysis of landings data (Murawski et al. 1983). Landings by commercial otter trawlers from 1977-1979 were clustered based on species compositions to provide definitions of fisheries in particular area/depth/month combinations with characteristic species mixes. A shallow-water group from Georges Bank defined from landings data (Fig. 1) was consistent with one defined from survey data by Overholtz and Tyler (1985) (Fig. 2). Trends in catch by that defined fishery were also consistent with trends in underlying populations over time. For purposes of system identification, the results are appropriate for defining systems of fishery operations in terms of time, space and catch composition. Those units can be reviewed in terms of practical aspects of management and record-keeping, or economic factors, depending on model of objectives.

Aside from defining site or species groupings in time and space, cluster analysis can be used to investigate other patterns of ecological or management relevance. To evaluate potential for seasonal co-distribution of young-of-year and older fish as a function of depth and temperature, Murawski (1984) applied cluster analysis with two age groups (age 0 and age 1+) of seven groundfish species as entities and mean depth and temperature of occurrence by calendar year as attributes(Fig.3). Results may provide evidence for potential technical interactions in trawl fisheries (similarity of yellowtail age 1 + and cod age 0) biological interactions (similarity of cod and haddock age 1+) or lack thereof (dissimilarity of silver hake age 1+, potential predator; and haddock, cod, yellowtail or winter flounder juveniles, potential prey). To evaluate heterogeneity in the diet of cod, Lilly and Rice (1983) used a nonhierarchical clustering approach and compared observed sizes of clusters with those simulated under extreme hypotheses of random and selective feeding, for example. Cluster analysis might also be employed to aggregate species having similar diets, beyond the pairwise summaries displayed in trellis diagrams (e.g., Grosslein et al., 1980). Richardson et al. (1980). concluded that transport may not be a major cause of larval fish mortality and recruitment failure along the Oregon coast, based on consistent annual patterns of species composition and distributions of larval fish assemblages from cluster analysis. The type of analysis could be expanded to include other components of the plankton community and seasonal contrasts.

Discussion

Choice of specific techniques for system identification is secondary to the definition of model objectives. Although this observation may labor the obvious, many criticisms leveled against techniques and interpretation are really criticisms of objectives. In the extreme, the value of "data snooping" and subjectivity sometimes associated with cluster analysis depends on the individual scientist's position on a "Cartesian ordinate."

The criticism of arbitrary criteria for group definition is not unique to cluster analysis, and can be minimized in several ways. In applying ordination procedures (Rice, 1985) the investigator must decide, for example, how much a component in principal component analysis reduces the total variance before it is meaningful or when a factor is reflecting a real pattern or just sopping up odd bits of variance. These problems can be ameliorated by 1) developing explicit criteria for pattern definition at the outset of the analysis, 2) identifying extrinsic information with which patterns should be consistent, and 3) applying additional alternative analysis such as ordination techniques.

For the example of geographical assemblage region associated with an assemblage production unit, the first criterion for accepting a pattern indicated by cluster analysis is that clustered sites be contiguous in space, and that site group boundaries persist over time. However, in the case where species groups are organized around environmental features with patchy distirbutions such as canyons or banks, continuous spatial distributions

might not be expected. Likewise, interannual shifts in boundaries, especially at small scales, may arise as a consequence of sampling intensity or sampling methodology (e.g., random station placement in a stratified random survey design). A particular area or category that is sampled intensively will usually form a cluster group. Categories with less data available may appear more variable or simpler and hence dissimilar. Areas of less intensive sampling will have shorter cumulative species lists. As a result, a boundary between the two areas may be created even though the species composition was consistent between both areas. Assemblage distribution patterns should be examined for consistency with other known ecological patterns, for example, circulation or production regimes, extent of water mass or sediment types. Finally, ordination or other analytic techniques can be applied. The advantages are at least twofold: any organization along gradients will be better identified using appropriate ordination analyses, and groupings (e.g., as factor loadings) from that analysis should reinforce what has been observed from cluster analysis, providing a "second opinion."

No single analysis will provide all the answers in a system identification. Of the problems in system identification listed earlier, cluster analysis cannot a priori distinguish environment, system and mechanism. Group formation indicates co-occurrence or similarity, but not interaction and causality. Although inputs and driving variables would be expected to be associated with the system they affect, the analysis provides no information on which is which.

The hierarchical quality of the analysis makes it a tempting tool for the aggregation of variables in a complex system. To simplify complex system models, species could be clustered over various functional dimensions (e.g., spatial distribution, trophic role, life history characteristics, operational co-occurrence in particular fishing gear) depending on model objectives and assuming assemblage behavior has been defined. The hierarchical perspective is also intellectually consistent with several approaches to theory and construction of system models, including the hierarchical perspective of Overton (1977) and definitions by Klir (1969). Klir describes five basic definitions of a system including (1) system resolution in time, (2) system trajectory over time, (3),(4) two similar types of holistic behavior, and (5) mechanistic behavior. Cluster analysis can contribute directly to the first four definitions. Mechanistic behavior, however, must be investigated through modeling and field experiments based on hypotheses generated from observing holistic behavior.

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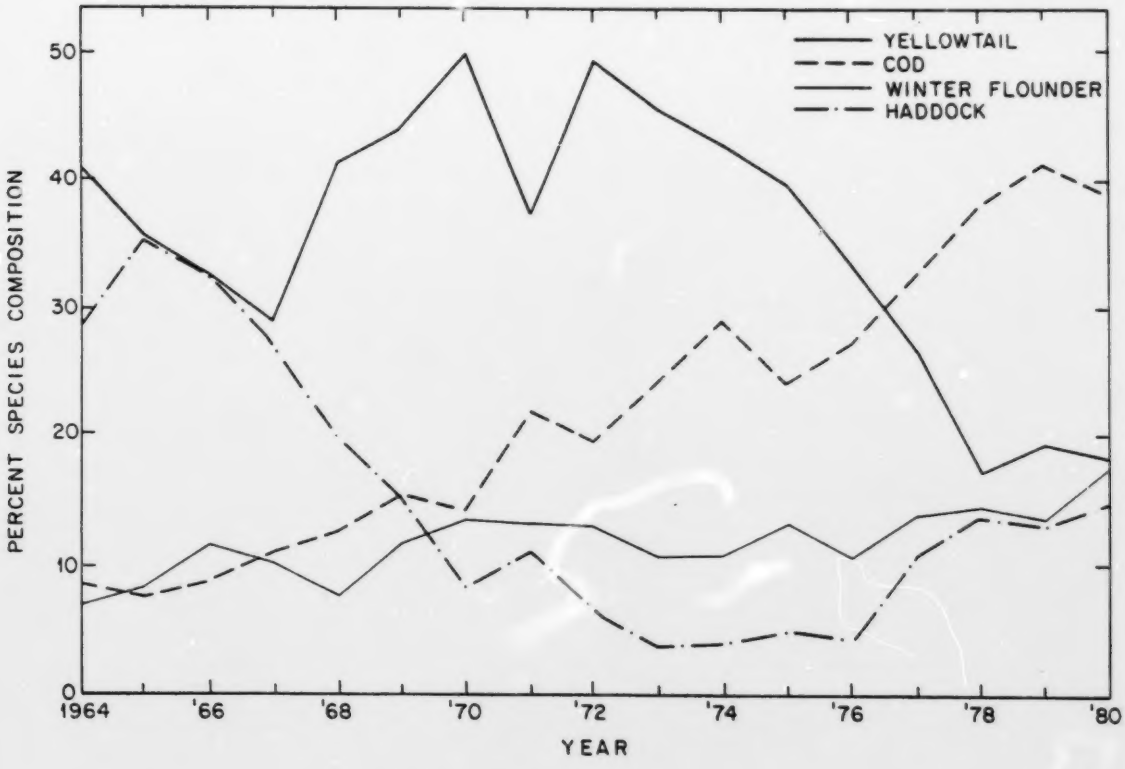

Figure 1. Percent species composition from Georges Bank shallow water fishery, 1964-1980 (Murawski et al., 1983)

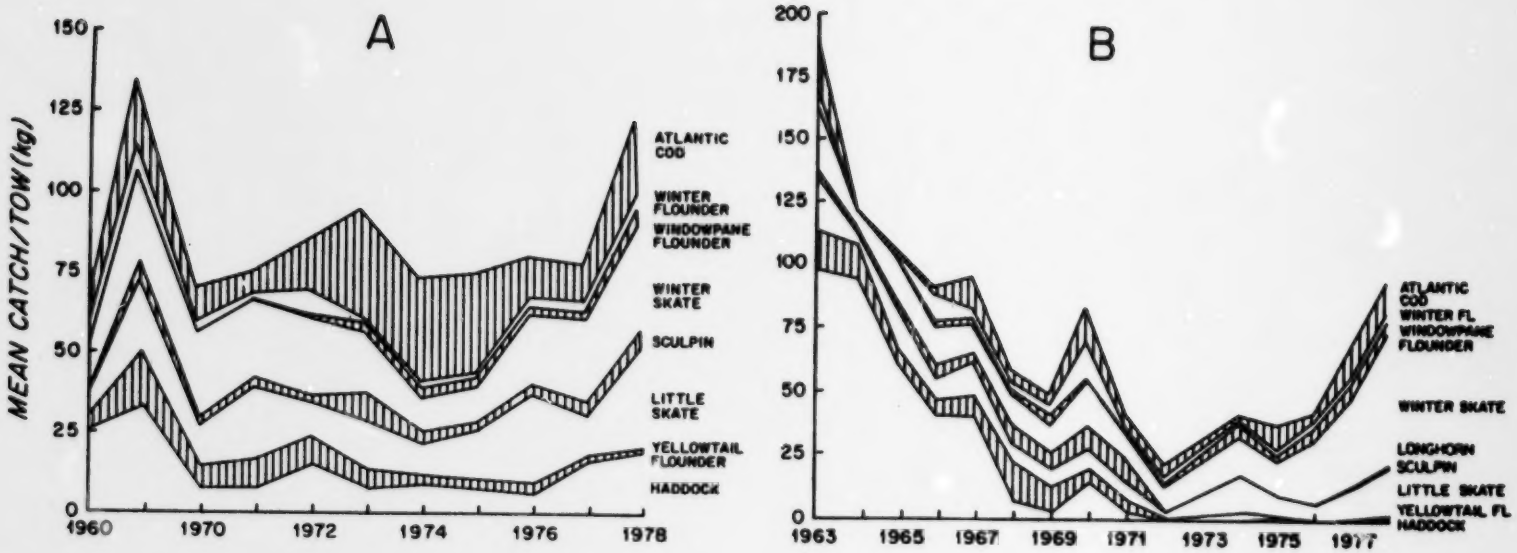

Figure 2. Response of species from Georges Bank shallow assemblage, 1963-1978; mean catch/tow (kg) in (A) spring 1960-1978, and (B) autumn 1963-1978 (Overholtz and Tyler, 1985). In part B, FL=flounder

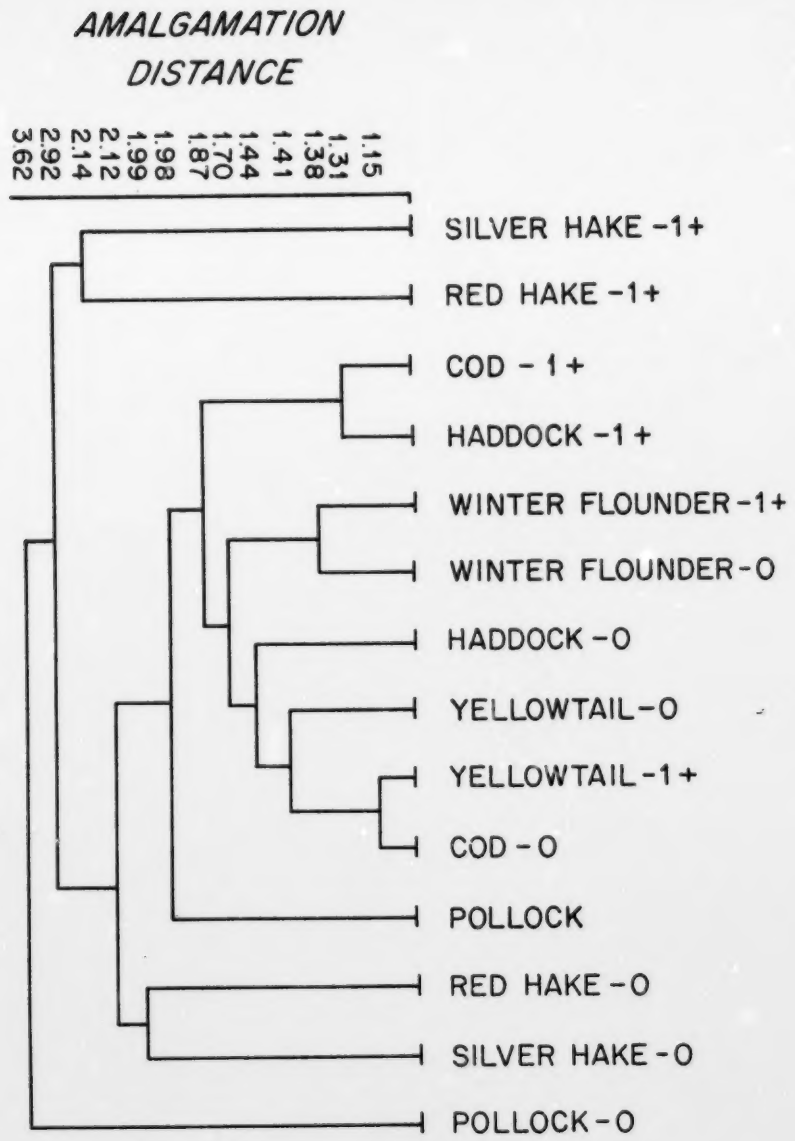

Figure 3. Clustering of Georges Bank groundfish species/ages based on seasonal depth and temperature affinities (Murawski, 1984)

Identification of Linear Ecological Models Using Time Series Methods

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Ordinary least squares (OLS) regression between an environmental variable, e.g. sea surface temperatures, and a biological variable, e.g. recruitment, is a common practice among fisheries biologists. This is usually an inappropriate approach for three reasons: (i) difficulties in model identification, (ii) type I errors are inflated because of autocorrelations (relationships appear to be significant when they are not), and (iii) OLS estimators are not efficient in the presence of autocorrelations.

Perhaps the simplest technique to mitigate the above problems is to use a Box-Jenkins transfer function model. These models are particularly appropriate if the true causal pathways are not known with certainty. This method is used to transform two series of data with complex autocorrelational and causal structure into two series in which the structure of the causal time lags can be readily identified. It should be noted that the resulting significance tests are slightly conservative (type II errors are inflated).

Box-Jenkins transfer function models are relatively easy to apply, and are readily available on all the major statistical packages. A word of warning, time series methods rarely yield reliable results with fewer than 40 data points.

A Brief Guide to Ordination Methods

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Several ordination methods are described briefly. Emphasis is placed on situations where each type of method is appropriate, and on the strengths and weaknesses of each method, rather than the mathematical properties of the methods. The appropriateness of any method depends on both the research question under study and the type of data available. A branching tree of questions is presented to aid in selection of an ordination method for specific inquiry.

Introduction

Fisheries research often involves working with large data sets; many cases, and each case possessing numerous attributes. Examples of such data sets include trawl surveys (cases are individual trawls, and the species are the attributes present in at least some of the trawls) and oceanographic studies (many stations, and a number of environmental attributes measured at each station). Ordination techniques are a class of quantitative methods which can be applied to such data sets. They result in cases and/or attributes being arrayed in a space of fewer dimensions than the original data set, but with relative positions preserved; that is, similar entities are close together and dissimilar entities are far apart.

Ordinations can be a useful step in a variety of inquiries. In our contexts, the commonest applications are in delimiting ecological units; either groups of taxa with similar distributions or groups of sites with similar species composition. Such applications are clearly related to our theme of identifying interacting marine systems. Other applications include identifying the number of dimensions of variation; that is, the number of distinct functional groups of species or sites; and extracting evidence for interactions among particular taxa and/or sites, or between taxa and attributes of the environment. Such analyses can also be useful steps in identifying interacting systems.

The ordinations themselves usually do not isolate units, but they provide an essential bridge from large data sets to smaller workable units. Graphs of ordination results often show gradient relationships, order and discontinuities among cases, and associations among attributes. Applications of straightforward statistical methods such as t-test and correlation to the ordinated data complete the job of rigorous, empirical identification of units and/or relationships in the original data, although some degrees of freedom are lost if tests are not planned *a priori*. However, final results are valid only to the extent that the ordination preserved the similarities or distances of cases and attributes in the original data. If necessary assumptions of particular ordination techniques are violated, ordinated relationships may appear simple and easily understood, but may be wrong. To use ordination techniques wisely, one must be aware of the diversity of methods which exist, and their strengths and weaknesses. Fortunately, most methods have been tested comparatively with simulated data, so sensitivity of each method to various violations of assumptions are well established. The books cited below provide good access to the comparative and simulation tests.

This technical report is intended as a simple guide to some widely used ordination methods. It emphasizes the relationship among the types of questions being investigated, the types of data being analyzed, and appropriate method of analysis. It does not provide a thorough mathematical treatment of the methods. Nor would all biometricians structure their guide exactly as this one is structured. Readers are encouraged always to seek both more detail and a second opinion. Many good reference books exist; I suggest Gauch (1982), Whittaker (1978), Green (1979), and Pielou (1984) as particularly relevant to ecological applications of ordination methods. This report draws heavily from the clear presentation in Gauch (1982).

Terminology

Ecologists commonly recognize 3 levels of ecological diversity; α diversity wich is the number and evenness of representation of species at a site; β diversity, which is the change in species composition and abundances of species from site to site; and α diversity, which is the change in species composition and abundances on large geographic scales (MacArthur 1972). In this paper, applications are exclusively at the β diversity level.

At that level, there are several measures of the amount of change in the fauna/flora among

sites. The Percent Similarity of sites j and k, with s species among the 2 sites, each species with abundance A_i, is measured as:

$$PS = 200 \cdot \sum_{i=1}^{s} \min (A_{ij}, A_{ik}) \; / \; \sum_{i=1}^{s} (A_{ij} + AA_{ik})$$

The commonest unit of change is the Half Change. The percent similarity of 2 samples 1 Half Change apart is ½ the similarity of two replicate samples. This is one of several instances where replicate samples from the same site enhance research design. Without replicate sampling, turnover rates must be calculated assuming the similarity of replicates is 1.0, whereas various sources of error often mean the true standard should be lower. Gauch (1982) provides further information on this and other measures of turnover.

I will be using the term community. I am not taking a side in the controversy regarding "assemblage" vs "community" (Tyler 1981), but following extensive precident in the literature on ordination methods. There community is used without presumptions concerning how or why groups of taxa or sites are associated.

From a table of species (s) by samples (n) data, 4 types of spaces can be calculated. In species space there is an axis for each species, and (n) sites are located in that s dimensional space. In samples space, there is an axis for each sample, and the (s) species are located in that (n) dimensional space (Fig. 1). For either of those spaces one may calculate the distance of all s-1 other species (or n-1 other samples) from each of the s species (or n samples). The distance from each of the s species (or n samples) can become axes in species (or samples) dissimilarity space. In the dissimilarity spaces the like species (or samples) are close to each other, unlike species (or samples) are distant (Fig. 2). The types of spaces are important, because some ordination methods require specific types of spaces, whereas other methods are appropriate for several or all types of spaces. Using an inappropriate method for a particular type of data may lead to serious distortions of relationships, and errors in interpretation. Again, Gauch (1982) develops the reasoning behind the different geometric representations of a data set in more detail.

Two artifacts will be referred to frequently: the "arch effect" and "involution" of gradient ends. The arch effect arises because many techniques, especially those based on eigenvector determinations, require that ordination axes be LINEARLY independent. If the community response to an underling influence is curvilinear, however, the eigenvector-based analyses extract only the linear portion of that relationship in the first axis. The nonlinear portion is both unexplained and necessarily nonlinearly related to the extracted axis. It commonly becomes the second or third "independent" dimension of variation. This can lead to serious misinterpretations of data, both because single nonlinear influences are erroneously treated as multiple independent linear ones, and because true independent additional influences on species occurrences are deferred to later axes, and frequently dismissed as unimportant. Simply graphing cases with the ordination axes as X and Y-axes usually shows whether or not an arch relationship is present.

Involution is more complex. Taxa may be non-monotonicly related to the underlying gradient: increase in abundance over some range of the environment factor, reach a maximum and then decrease over yet higher (or lower) values. Especially at the ends of the gradient, when species distributions are only partially sampled, cases separated from each other on the true gradient may resemble each other in abundances, representing similar positions on opposite shoulders of the abundance curve. Ordinations may then "curl" the ends of the gradient back on themselves, placing sites with different underlying properties incorrectly close. This problem is difficult to see in quick inspections of the ordination results. When data have high β diversity, techniques resistant to involution should be considered, and distributions of species abundances be plotted as functions of the ordination axes.

Ordination Methods

The various ordination methods will be presented using a branching set of questions regarding both research objectives and properties of available data. The branchings do not exhaust the ordination methods which have been proposed, and some techniques appear at more than one branch (Fig. 3). The branching sequence should be adequate for a researcher with a general question and familiarity with basic distributional properties of the data to select an appropriate ordination method or methods. If a researcher has neither a question nor familiarity with basic distributional properties of the data, ordination methods (among other statistical procedures) are not appropriate.

Do Research Questions Concern Differences Among Preidentified Groups?

Discriminant analyses

When research questions involve the form or magnitude of differences among preidentified groups, some form of discriminant functions analysis (DFA) is often the most powerful analysis. A number of versions of DFA, and analyses similar to DFA, have been developed. I

will not elaborate on them, because many references do not consider DFA to be a true ordination technique. However, because DFA methods are misused frequently in ecological applications, I will explain several of the common pitfalls potential users may encounter. For additional detail, Tatsuoka (1972) presents a particularly clear introduction to discriminant functions analysis.

DFA has 2 types of applications: to test for statistical separation of known groups, and to classify cases into previously described groups. The applications are not the same conceptually, they differ in mathematical detail, and they differ in sensitivity to violations of underlying assumptions of DFA.

Used as a tool for testing differences among groups, DFA addresses questions such as: are the groups significantly different, and how much does each attribute contribute to the differences among groups. DFA can position groups and cases in discriminant space, similar to common ordination techniques. These positions have been interpreted directly as reflecting various properties of ecological niches, including niche breadth and niche overlap of species (Dueser and Shugart 1979).

Used as a tool to investigate separation of groups, DFA is sensitive to the assumptions of multivariate normality and especially of homogeniety of variance-covariance matrices among groups (Williams 1981, 1983). If these assumptions are violated neither the magnitude of within-group dispersions of cases nor the relative distances among group centroids are maintained in the reduced discriminant space. These distortions are important, because serious errors are introduced into interpretations of results. Levels of statistical significance and functional interpretations of how groups differ and relative positions of groups in space, are all distorted. Because the requirement of homogeneous variances among groups is often violated in ecological data (especially if groups differ substantially in numbers of cases) scientists should be extremely cautious in applications of DFA to testing differences among groups (Williams 1983).

DFA is much more robust when used as a tool for classifying cases into known groups. If DFA is used to classify the same data set that was used to estimate its parameters, accuracies of classification are overestimated (a common occurence in statistics). Several methods exist to overcome this problem (Harner and Whitmore 1981), and many research questions can be answered with approaches stressing accuracies of group classification, rather than separation of group centroids (Williams 1983, Rice et al. 1983). When accuracy of classification is used, the distribution of classification errors is important. Ideally, errors are distributed among groups in proportion to the sample sizes of the groups (Table 2a). When one group is predicted less accurately, it could mean that the groups truly differ, but not all suitable areas are occupied (Table 2b), or that groups do not differ in the attributes measured, and the analysis has simply tried to characterize the largest group (Table 2c).

Early applications of DFA stressed quantifying differences among species. However, often several species are sensitive to similar environmental influences, and distributions overlap to varying extents. Therefore, such applications often resulted in interpretations stressing differences among species which were actually relatively unimportant determinants of overall occurrence of the species (Raphael 1981). Many research questions involving predicting the occurrences of species of interest from environmental and/or physiographic data can be better addressed by discriminating sites where a species does occur from sites either selected at random, or sites where the species does not occur within the general region (Rice et al. 1983).

Do questions relate specifically to responses of species to known environmental gradients? Direct gradient analysis

There are two main types of questions which are investigated with direct gradient analyses. The first type inquires what environmental factors influence the distributions and abundances of taxa, and to what extents. The second type asks how can environmental factors most meaningfully be measured. Because biological responses to many influences are nonlinear, the latter type of applications seek units of measurement defined by the biota. Equal relative changes in abundance (or other response) by a number of species to various segments along an environmental factor imply an interval continuum along that factor that may not correspond to units of the environmental measure itself. These methods can also be used to check for common limits of distribution along an environmental gradient, or to investigate questions regarding the presence of discrete communities or assemblages of independent species. The major drawbacks to Direct Gradient Analysis are lack of objectivity (the important dimensions of change are preselected), and the requirement of prior collection of both biotic and environmental data.

For any inquiry about the responses of species to environmental factors, it is appropriate (and often informative) to plot the data. Assuming all assumptions appropriate to each specific application are observed, visual inspection can be accompanied by regression analysis (Draper and Smith 1981) in many cases. In addition, there are 2 classes of direct gradient analysis; selection between them depending on the shape of the species response to the environmental factor.

No assumptions about species distribution along th environmental gradient:
Weighted averages

The Weighted Averages ordination method uses previous knowledge about the relationship of a few species to the environmental gradient in order to derive an ordination of all species and sites. First a few (commonly 3 to 5) species, usually characteristic of one extreme along the gradient, are selected. For example, 4 species characteristic of "deep water", may be chosen. These taxa are given high weights W_i (scale is arbitrary). Then for all the j sites, scores (S_j) are calculated as:

$$S_j = \left(\sum_{i=1}^{s} A_{ij} \cdot W_i \right) / \sum_{i=1}^{s} A_{ij}$$

where A_{ij} is the abundance of the ith species at the jth site. Measures of species responses other than abundance can be used. When scores for all the sites have been calculated, new weights are calculated for each species. These W_i are the average scores of all sites supporting each of the i species. These new weights (now one for each species) are used to calculate a second round of site scores S_j. The resulting ordination scores often are an intuitively satisfying representation of the responses of the species in the community to the environmental influence. For example, Curtis and McIntosh (1951) extracted a full successional gradient using Weighted Averages, starting with 4 species assumed to characterize mature forest.

Weighted average ordinations can be done with more than one environmental gradient. For several gradients, however, the computations become complex, and results approach those of correspondence analysis. In such cases the latter technique usually is recommended. Ordination axes of weighted averages are always ordinal; whether or not they can be treated as interval depends on the original species by site data matrix and the distribution of sites along the axis.

Species distributions are normal or log-normal:
Gaussian ordination

Several methods of direct ordination have been proposed for studies assuming a Gaussian response of species to environmental factor. Although Ihm and Groenewoud (1975) may be best known, it is included in the more general method of Gaussian Analysis proposed by Gauch et al. (1974). Data are the abundances A_{ij} of the species (i) at each site (j) and measures of the environmental factor X_j at each site. More than one factor can be studied, each on a separate axis. A_{ij} are fit to the equation:

$$A_{ij} = A_{i,max} \cdot e^{(X_j - \mu)^2/2\sigma^2}$$

where $A_{i,max}$ is the maximum abundance of species i, μ is the point on the environmental gradient where the maximum abundance occurs, and σ^2 is the variance of the species response around that point. These 3 parameters are estimated for each species on the first environmental factor. For each additional factor considered, 3 additional parameters are estimated: the optimum position of each axis (μ), the variance (σ^2) around that point, and the orientation of the angle made between the new axis and the existing ones. $A_{i,max}$ does not change, of course.

In terrestrial studies, Gaussian Ordination has proved useful and powerful at capturing species-environment relationships. I know of few marine applications, but studies covering underlying marine gradients are more difficult. Several basic ecological insights have arisen from Gaussian ordination; such as across several species the $A_{i,max}$'s are themselves lognormally distributed (Preston's "Octave Scale", Preston 1948, 1980), the σ^2's are normally distributed with their standard deviation = 0.3 of their mean, and the angles of intersection of separate factors are rarely either 0° or 90° (i.e., interactions among environmental influences are the rule, not the exception). The scatter of optima (μ's) of a number of species along a gradient depends on the specific application: sometimes μ's are clustered (several species share similar optimum conditions), and sometimes they are scattered (species optima are independent or possibly determined by competitive interactions among taxa), implying different types of community organization.

Considering robustness of application, Gaussian Ordinations are tolerant to modest departures from true Gaussian underlying distributions: the true μ and σ^2 can be extracted from data with both moderate skew and bimodality. This is both a strength (the power of the analysis method, and its direct interpretability, can be used with less than ideal data) and a weakness (the method can provide apparently reasonable fits to data where the underlying distributions are actually nongaussian). The method performs poorly when most samples are clustered from only a few positions along the environmental gradient; it works best with samples evenly spaced along the gradient.

The tests are not with known environmental gradients: Indirect gradient analysis

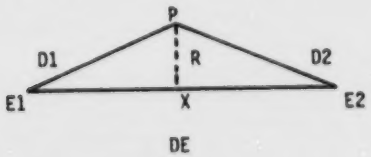
Are the number of underlying gradients known?
Yes:

Are attributes which characterize extreme conditions known?
Yes: Weighted Averages (previously described).

No: Are samples which characterize extreme conditions known?
Yes: Polar Ordination.

Polar Ordination methods are sometimes referred to as "Bray-Curtis Ordinations", but Bray-Curtis methods are one type in this large class. The ordinations are based on intersample distances defined by species, and hence are done in species space. It is possible to ordinate interspecies distances in sample space, but proper interpretations for the results are often less clear. When proposed, Polar Ordinations were designed largely to avoid using the concept (and computationally, the value) of "multivariate centroid", a key step in many eigenvector ordination procedures. The objection to the use of centroid is that the notion of a locality "average" to all species (or a species "average" in all ecological requirements) is a bizarre starting place for a community analysis. Although this objection rarely arises any more, polar ordinations are still used frequently, because they make few assumptions about distributions of data, and provide clearly interpretable results in many applications.

In polar ordinations, "double standardization" (actually a scaling, and not a standardization) is almost always done. The maximum abundance of each species is set to 100, and abundances at all other sites are scaled down. Then the total abundance of all taxa at each sample is set to 100; the individual abundances again scaled. After standardization, the endpoints (E1 and E2) for the ordination are selected; i.e. the samples which characterize extreme conditions on the gradient of interest. (Note that unlike direct gradient analyses, no measures of the gradient itself are required). Next the dissimilarity of each sample from each of the endpoints is calculated. Using the 2 dissimilarities (D1 and D2) of a sample to each endpoint, and the dissimilarity of the endpoints themselves (DE) the position (X) and deviation (R) of each sample (P) can be calculated geometrically:



DE

If the deviations R are large, additional axes, with new endpoints, can be calculated. Positions of cases on the axes are always ordinal; if the original data allowed the dissimilarities to be interpreted as interval measures, the ordination positions can also be treated as interval.

Polar ordinations have several strengths. Because the endpoints are fixed, polar ordinations are resistant to involution, and the axes are resistant to distortions due to either clustering of samples along the gradient or presence of outliers (unless an outlier was selected as an endpoint). The polynomial relationship between the first and later ordination axes (the arch effect) is also minimized by the fixed endpoints. The Pythagorean geometry used in the computations also reduces the effect of nonlinear relationships between dissimilarity measures and actual sample separation, and between the attributes used in calculating the dissimilarities. Polar Ordinations are good for β diversity levels up to 5 Half Changes. Thereafter, performance deterioriates quickly. The method is also computationally cheap, in that only dissimilarities from the selected endpoints need to be calculated, rather than the entire sample by sample dissimilarity matrix.

The major weakness of Polar Ordinations stems from the preselection of endpoints, and hence lack of objectivity: what one chooses to be important is important. There are other problems as well. When 2 or more axes are selected, the axes have no necessary orientation to each other (orthogonal or otherwise), so multiple axis interpretations are compromised. Also, although Polar Ordinations can be done with either the species or the samples in a data set, the analyses are wholly separate. Therefore, they have nothing interpretationally in common. That means that explaining positions of samples by interactions of species (or vice versa) can only be done intuitively; the results of one analyses are not directly applicable to the other analysis. Finally, because of the deterioration of performance with high β diversity, Polar Ordinations become inappropriate when more than a few sites have no species in common.

No: Neither taxa nor sites characteristic of extreme conditions can be specified. Multi-dimensional scaling

Multi-dimensional scaling (MDS) includes a range of non-metric methods for ordinating data. Because of the differences among approaches, I shall not give a summary, beyond the generality that these methods replace the actual dissimilarities or distances with the rank-order of the dissimilarities or distances. Given the rank order of distances of n samples based on the entire s dimensions, MDS methods attempt to position the samples in a space of many fewer dimensions while preserving the ordering present

in the larger dimensional data set. How this is done differs substantially among methods, with various methods emphasizing different things. Because MDS uses only rank- order information, it is particularly suitable for analysis of data where nonlinear relationships among attributes are common. For example, it is an alternative to Principal Components Analysis for data of low β diversity. It is also suitable for cases where one knows the number of dimensions of interest, but does not want to specify endpoints of axes. MDS axes commonly are treated as interval.

MDS has a number of strengths. The most appealing attributes are that MDS makes few and realistic assumptions about the data. It is appropriate for ordinal as well as interval starting data, as long as monotonic rank orderings are possible. Because assumptions of linearity are often violated with ecological data, the freedom of MDS from such assumptions is attractive. MDS also avoids subjectivity in choosing either cases or attributes for special emphasis. Although an initial ordering of cases is assumed, MDS proceeds interatively towards convergence on one of several criteria, depending on the method. The initial ordering of cases influences only convergence time, and not the final answer. Hence MDS can be used in many cases where polar or weighted averagrs might be appropriate, but users wish to avoid subjectively choosing the important dimension of variation.

The primary drawback of MDS is its computational demand. Computational requirements increase as the square, cube or greater power of the number of cases, so application to large data sets is often prohibitively expensive. Similarly, because the method proceeds interatively, one may become stuck in a local optimum with data which are noisy or multimodal. The clustering of samples along the final gradient flattens the rate of approach to optimum, increasing both convergece time, and the problem of local optima. Other drawbacks include the use on only rank-order relationships. If more metric information is available (often collected at substantial additional cost), no use is made of it. MDS is usually used to ordinate samples by distance in species space, but one can also ordinate species, rank-ordered by distances in sample space. Both cannot be done together, however, so simultaneous interpretation of species and samples is not possible.

No: The number of underlying dimensions is unknown

Is the β diversity greater than 2.5 half changes?
Yes:

Correspondence analysis (Reciprocal averaging)

Correspondence analysis (CA), or reciprocal averaging, can be introduced as either an interactive geometric extension of Weighted Averages Ordinations, or an eigenvector based relative of Principal Components Analysis. Because of its parallels to both Weighted Average and Principal Components methods, CA is widely applicable. The term "reciprocal averaging" is applied because species scores are averages of the scores of the appropriate sites, whereas site scores are averages of the scores of species.

Procedurally, the data set is first scaled, as in Polar Ordinations, so both species and samples range from 0 to 100. Every species is assigned a unique score on a 0 to 1 scale. Initial assignment can be done in any manner; in practice, usually at random. Final scores are independent of this initial assignment, although convergence time is not. Next the scores of all species at each site (weighted by scaled abundances) are averaged, producing a series of site scores. New species scores are calculated as the average of scores of all sites where the species occurs. The cycle is repeated until both species and site scores converge. In practice convergence commonly requires 3-7 interations. The parallels with Weighted Averages is clear from the treatment of species and sites scores. As an eigenvector technique, the major differences from Principal Components Analysis are the dual scaling of species and samples (in PCA only variables are standardized, rather than scaled), and the use of a χ^2 measure of distance of sites and species, rather than the correlation or variance-covariance matrix used in PCA. The procedure is repeated for additional axes, with relative sizes of eigenvalues determining scales of axes and the number of axes extracted.

CA has many strengths. Results are provided simultaneously for cases and attributes, so unitary explanations of species and site relationships are straightforward. Axes are interval scale, and appropriate for further statistical analyses. The method can be applied to either original case by attribute data sets, or to dissimilarity matrices (species or sample dissimilarity spaces). It assumes neither linearity nor monotonicity of relationships among attributes, and is robust to moderate noise levels. Because CA does not assume monotonicity, the entire range of a species (increasing, plateauing and later decreasing in abundance along a gradient) can be bracketed by the analysis, and CA works well up to 5 to 7 Half Changes. It is resistant to presence of outliers, to positioning of samples along the final gradients, and to involution of ends of gradients. It is also computationally efficient, with demands a linear function of the number of cases. It is also wholly objective; no cases or attributes are selected as special.

The major weaknesses of CA arise from its assumption of a symmetry of species and samples; i.e. the distribution of one species across all samples should resemble the distribution of all species in any sample. This computational symmetry means rare species present in samples

with few species are especially distinctive, and influence the analysis strongly. Also because the distributions of species at the ends of the gradients are truncated (at the end of the range of samples, some species abundances are >0), cases at the ends of the gradients are packed together. Cases in the middle of gradients, where the entire distributions of characteristic species are captured in the full data set, are more widely spaced. Finally, RA is vulnerable to the arch effect, so secondary gradients may not appear in the 2nd or 3rd axes.

The tendencies of RA to arch when true species responses to gradients are curvilinear, and to condense sample placements at the ends of gradients led to the development of Detrended Correspondence Analysis (DECORANA - Hill and Gauch 1980). DECORANA adds two additional steps to common CA. After each iteration except the last, the ordination axis is divided into segments. Within each segment scores are centered separately. Although within each segment of an axis an arch may persist, across the entire axis there is no trend: within segment averges are identical. Thus curvilinear responses are transformed to linear ones, so not only are the ordination axes not linearly correlated, but they have no systematic nonlinear relationships either. This modification appears to be effective at eliminating the arch problem from CA analysis.

DECORANA also attempts to diminish the condensing of cases at the ends of gradients. Within each segment the SD of species scores are calculated for each sample. Segments are then adjusted so that the average SD's are the same across segments. This step streaches the ends of the gradients, where species distributions are truncated, affecting the calculated average SD. The step puts the ordination axes in units of SD's of species turnovers (for "good" data 1 SD of turnover = 1.35 Half Changes), so equal distances on an axis reflect equal amounts of change in species composition. These additional steps require original data: DECORANA cannot be done with only species or sample dissimilarity spaces. Also, because of the range truncations, the actual positioning of species is not captured quite as well as the spacing of samples. DECORANA maintains all the other strengths of CA.

No: diversity is <3 Half Changes: Principal components analysis (PCA) and relatives.

Like CA, PCA can be introduced either as an eigenvector method or geometrically. As an eigenvector technique it can be done with either a correlation matrix or a variance-covariance matrix; either derived from a cases by attributes data set. With a correlation matrix, relationships of attributes are assumed exclusively linear: with a variance-covariance matrix, multivariate normality is assumed. In either case, results are most meaningful when data are standardized (the variable mean is subtracted from each measurement, the result divided by the SD of the variable). Standardization is especially important if variables differ substantially in scale.

Geometrically, consider a cloud of samples in species space (Again, data are usually standardized). The first PC is selected as the axis through that cloud which minimizes the sum of squared deviations of samples from the axis. In PCA distances are implicitly Euclidean distances:

$$d_{jk} = \left[\sum_{i=1}^{s} (A_{ij} - A_{ik})^2 \right]^{1/2}$$

Additional axes are selected which successively minimize the residual sum of squared deviations of Euclidean distances from all previous axes. Axes are interval scale, and positions of cases on each axis (the PC scores) are normally distributed. Also, the cosines of the original attributes with the PC axes give direct measures of the relationship of the attributes to the new Principal Components.

Interpretability of results can be improved by various rotations of the components: Varimax rotations emphasize simplifying the attribute loadings (each attribute should have either a very high or very low weighting on each component); Quartimax rotations emphasize simplifying the component structure (each PC should have as many attributes as possible with either very low or high weightings); Equimax rotations try to balance those two objectives. If one's primary interest is in relationships among the original attributes, Varimax is often recommended. If one's primary interest is in characterizing the underlying gradients, Quartimax may be more appropriate. The individual PC's, original or rotated, are orthogonal. If one expects important gradients influencing the species distributions to be correlated, oblique rotations are possible. Oblique rotations are uncommon in ecological studies, possibly due to difficulties in interpreting results.

The major strength of PCA is that it preserves perfectly distances among samples. For analyses of fine scale relationships among sites it is the most powerful technique, and mathematically well grounded. It is purely objective, and allows simultaneous interpretation of both species and sites.

The major weaknesses of PCA arise from the requirements of only linear responses of species to the underlying factors, and of multivariate normality of data. These assumptions are

violated with species distributions which are Gaussian, or with abundance data which are zero for some species at some sites. Hence the method can only be legitimately applied to data of low β diversity, where all species are present in nearly all samples. The strong linearity assumption also make PCA prone to both the arch effect and involution of endpoints. Although the PC's are mathematically orthogonal, biological independence does not necessarily follow. If the assumptions of linearity or homogeniety of variances of attributes are violated, biologically interrelated things can be ordinated on different components. Also, random number tables give superficially "reasonable" PC's (Karr and Martin 1981), so tests for significance of PCA's are essential (Thorndike 1978). The weaknesses of PCA are serious. Thought should always be given to substituting DECORANA or MDS for PCA, unless all assumptions are met, and the fine structure of relationships among cases and attributes is important to the interpretations of results.

Two additional eigenvector ordination methods are used in some specialized situations: Factor Analysis and Principal Coordinates Analysis. Both share many properties with PCA, including most of the weaknesses of PCA for many ecological studies.

Although it can be used in exploratory, studies factor analysis, unlike PCA, often assumes some explicit underlying "model" for the data; variation is attributable partly to systematic influences of the "factors" in the model, and partly to noise. It focuses on accounting for the covariance structure of the data, rather than explaining the variance terms. The amount of noise is estimated in some way (frequently internally to the analysis, using the "communalities" of the variables in the analysis). The diagonal elements of the correlation or variance-covariance matrix are reduced by the estimate of noise (in PCA, the diagonal elements of the correlation matrix are 1.0: a variable is perfectly correlated with itself), before the eigenvectors are extracted. Thereby, the underlying factors of the model are pulled out of the data more cleanly. Rotations to simple structure are usually an important part of Factor Analysis. Factor Analysis has not been used widely in ecological studies, possibly because patterns of variance as well as covariance are of substantial interest in many applications.

Principal Coordinates Analysis differs from PCA in focusing exclusively on preserving distances among samples in species space. In the process relationships among attributes (the PC loadings) are deemphasized. Focusing exclusively on intersample dissimilarities allows Principal Coordinates Analysis to be performed in species dissimilarity space as well as in species space. Unlike PCA, it is also suitable for binary or ordinal data, as long as a distance matrix of samples can be created. Principal Coordinates Analysis trades off the ability to simultaneously interpret case and attribute relationships for greater robustness to violations of distributional assumptions and types of suitable data. The assumption that all cases form a single cloud of points remains, however. If there are large distances between groups of cases, the detailed structure within groups is overwhelmed, making the precision of placement of cases on the axes (the "coordinates") deceptive. Hence Principal Coordinates Analysis is suitable only for data of low β diversity.

Summary

In this overview I have touched on a number of ordination techniques. Is there a best buy? The answer is clearly no: the best technique depends on properties of the data and the questions under investigation. Calling an analysis "exploratory" is not an excuse to misanalyze data or misinterpret results through flagrant violation of important requirements of any specific technique. Results of "exploratory analyses" have been shown to entrench the preliminary pattern in a researcher's mind, influencing greatly subsequent analyses and interpretations; it is important that these first patterns be real ones. In practice, few researchers follow all restrictive assumptions of a technique rigorously. It is still important to know what the requirements and pitfalls of any analysis are, and what artifacts are likely to arise from not meeting the requirements or falling into the pits. One is then at least able to recognize the artifacts, and avoid basing "biological interpretations" on them.

Commonly, analogies are made between areas of wide, free selection and a candy shop; a restricted choice among many pleasing things. With ordination techniques, a better analogy is with a butcher shop. What one takes out often becomes the main course, so quality matters. In skilled hands, commonplace cuts of data can be made as elegant as a de-boned chicken; in careless hands, a prime piece of work can be ruined. And even the best butchers have a few scars of past mistakes.

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Table 1. Hypothetical data for 3 species (1, 2, and 3) and 3 samples (X, Y, and Z), arranged in a typical case by attribute table.

Samples	Species 1	2	3
X	5	2	1
Y	6	9	15
Z	10	15	25

Table 2. Examples of different error distributions from classification step of a discriminant analysis. Case A is most consistent with DFA assumptions; case B may reflect undersaturation of suitable areas by a species; case C reflects no actual difference among groups, although in all 3 cases classification accuracy was 90%

Site Actually Was:	Site was classified as belonging to group:					
	Absn.	Pres.	Absn.	Pres.	Absn.	Pres.
Absent	68	7	65	10	75	0
Present	3	22	0	25	10	15
	CASE A		CASE B		CASE C	

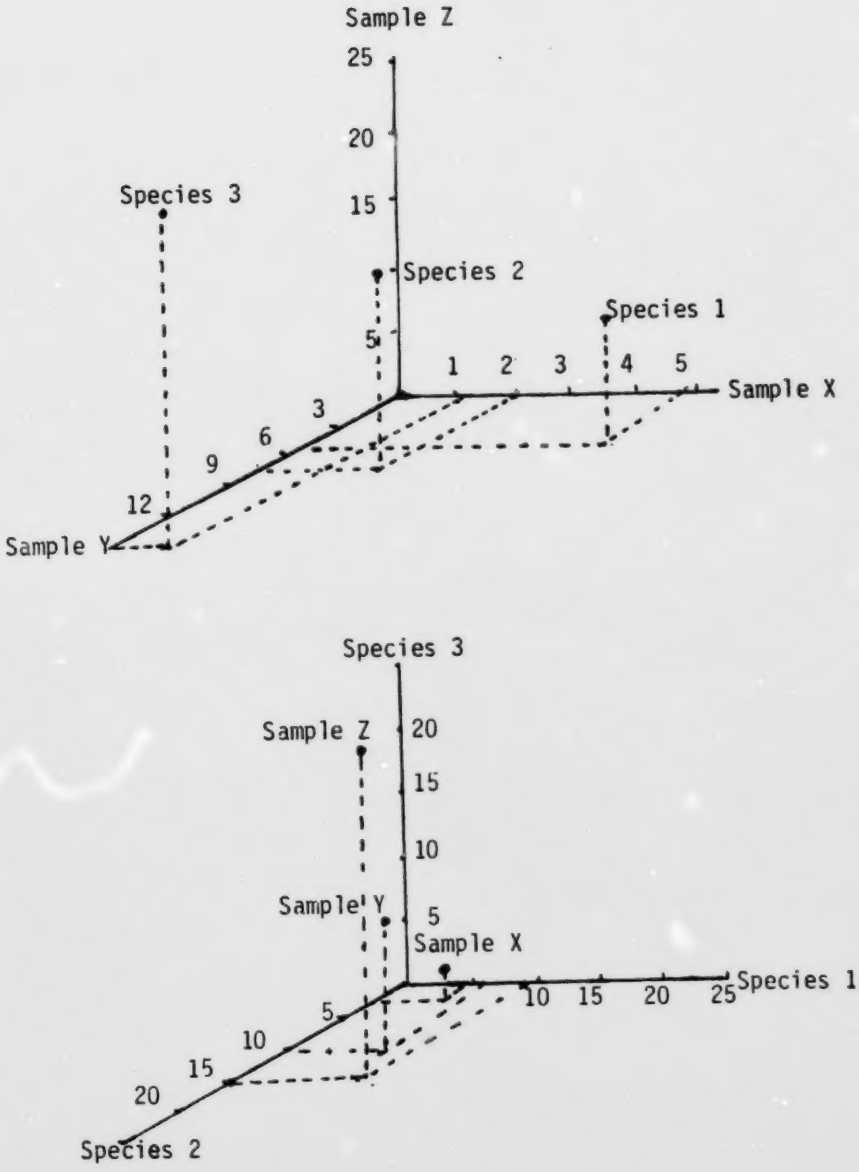


Figure 1. Illustration of species positioned in sample space (top graph), and samples positioned in species space (bottom graph); both for the data in Table 1.

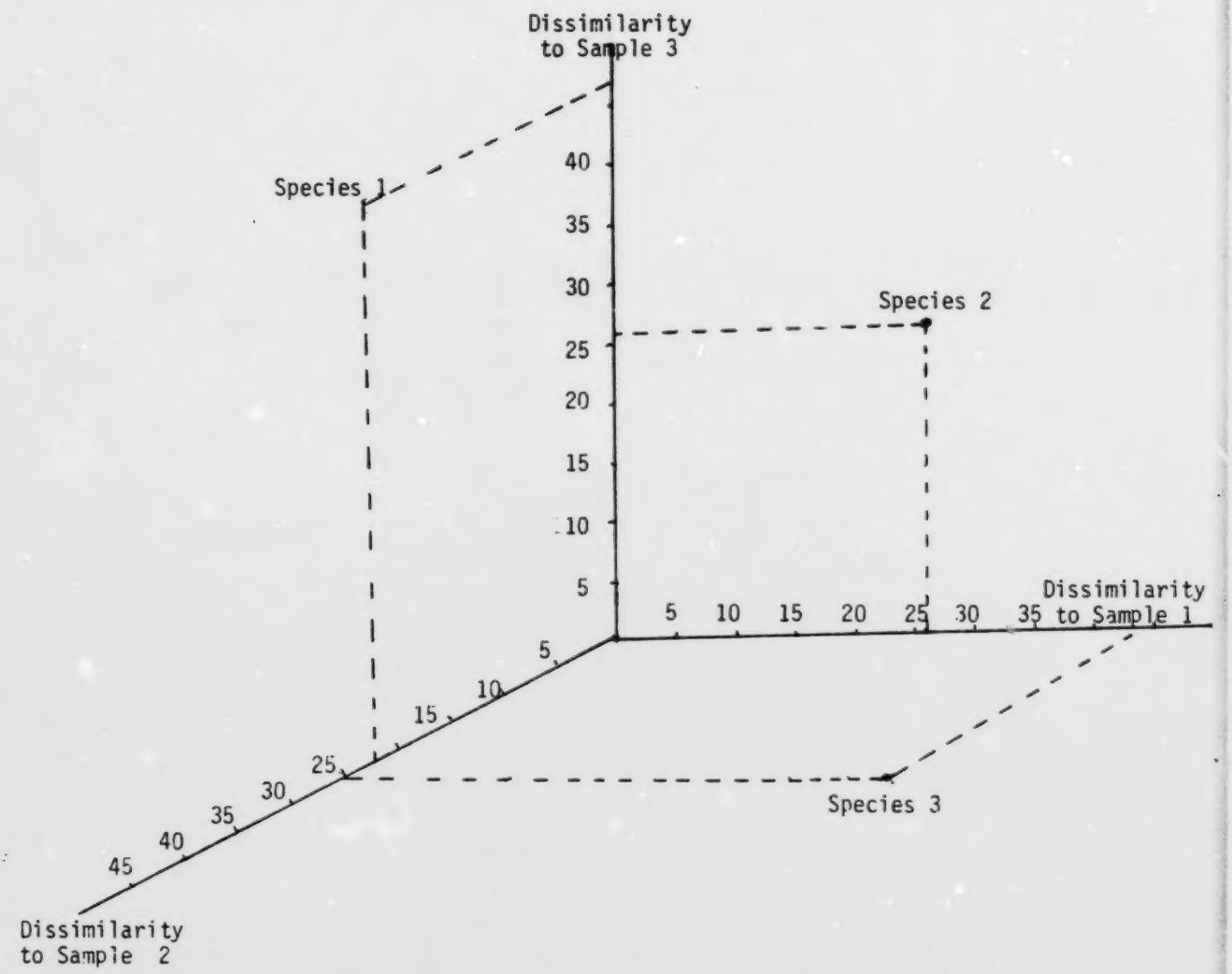
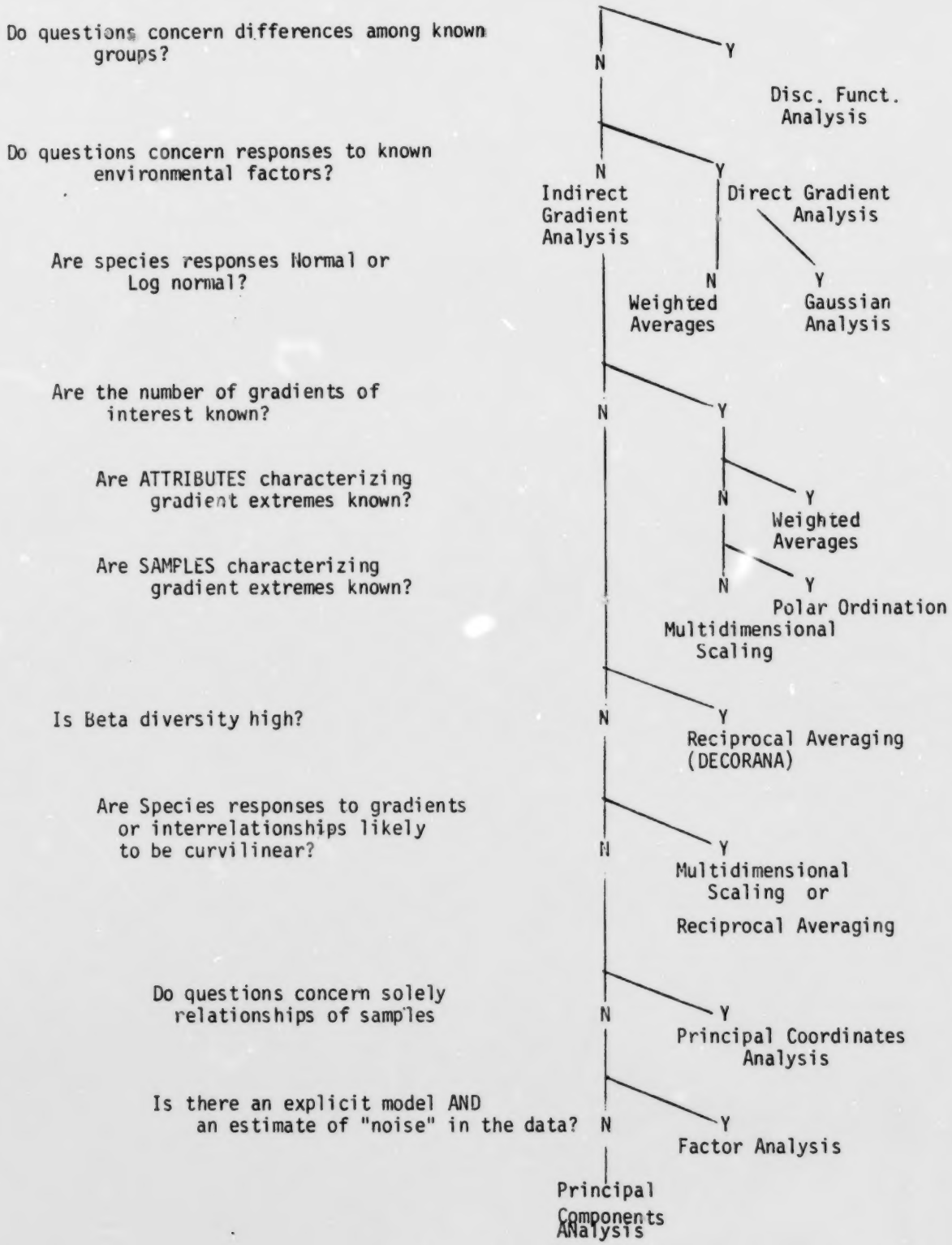


Figure 2. Illustration of species positioned in sample dissimilarity space, using the data from Table 1.

Figure 3. Branching of Questions for selection of suitable method of ordination.



Model Uniqueness

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Silvert, W. 1985. Model Uniqueness. p. 132-135 In: R. Mahon [ed.] Towards the Inclusion of Fishery Interactions in management Advice. Can. Tech. Rep. Fish. Aquat. Sci. No. 1347.

Ecosystem modellers have traditionally thought in terms of unique models. When more than one model is proposed for a particular ecosystem it is generally believed that at most one can be "correct" and that the rest should be promptly found out and "falsified". Recently ecologists have begun to recognize that this modelling philosophy, which is rooted in the physical sciences and engineering, is not always appropriate for ecological research. The relative paucity of data and the absence of a comprehensive set of fundamental laws make it difficult to falsify models, while increasing use of modern methods of system identification have led to increases in the number of models which may be proposed for a given ecosystem. Managers in particular have come to recognize that it is unrealistic to expect scientists to agree on a single model, so they have taken to using relatively arbitrary methods for selecting management models. A better approach might be to break with the present insistence on unique models and to develop ways of coping with the practical reality that ecologists cannot in general determine which of several conflicting models is most likely to be correct, and that managers need to recognize that any of these models might be valid.

Introduction

Ecological modelling began as offshoot of engineering. The pioneers in the field used the same methodology, even the same symbols and nomenclature (e.g., Odum). This has had a significant effect on the way we do our modelling, and it may be time to reevaluate ecosystem modelling in light of research in other fields to see whether the engineering approach is still the best for what we want to do. The growing popularity of system identification methodology has led to significant changes in the modelling being done in other fields, such as economics and the social sciences, and it has also had a major impact on some branches of engineering.

A key distinction is that ecologists are basically researchers who are trying to find out how ecosystems work, and this is true even of ecologists whose duties are focussed on fisheries management or other specific applications. Engineers, on the other hand, do not need to discover the laws of nature -- they take them as their starting point. They codify them and translate them into mathematical equations or perhaps computer code. For example, an aeronautical engineer begins his career with a basic knowledge of gravity and aerodynamics, which he uses to design planes. He doesn't do basic research on these subjects.

The typical engineering approach is to include everything in a model. This "kitchen sink" approach is practical because data and parameter estimates are usually available. Consequently engineering models are often massive computer simulations which appear to possess awesome complexity. However, size alone is not the same thing as complexity. The forms of interaction are usually simple -- even NASA's biggest models are all based on Newton's laws. However, as Halliday and Pinhorn (1985) point out, models with lots of parameters are not readily accepted by fisheries managers, often for the good reason that reliable parameter estimates are not available.

Modelling Approaches

The success of the engineering approach in biology has generally depended on how well understood the underlying laws of nature are. In molecular biology application of laws of chemical bonding has led to spectacular advances. Physiologists also deal with models of this type with moderate success. But the value in ecology is not so clear, and marine ecology, where data are exceptionally costly and difficult, is perhaps the extreme case.

In ecology we not only don't have a well-defined set of laws governing interactions, we often do not even have a set of working hypotheses!

It is commonly believed that some statistical methods can be used in the absence of a model. While some methods are more robust than others, even a cursory glance at the statistics literature will confirm that "statistical" models have just as much structure as anything else. Often the use of simple linear models leads to absurd results, such as biological models where the number of surviving larvae can exceed the number of eggs with which you start.

Statistical approaches have been most successful when based on models having a foundation in biological theory. For example, the early studies by Sutcliffe et al. of the effects of environment on fish stocks were based on the idea that these effects would be most pronounced at a particular stage in the life history, and in fact the result of lagged correlation analyses confirmed that the appropriate lag generally corresponded to the earliest life stage. Current techniques of time series analysis, such as those discussed by Myers (1985), are more sophisticated but conceptually derive from the same basis.

A common problem with the use of statistical models is that structural interactions are not always reflected in statistical correlations. For example, if we look at a simple plant-herbivore-carnivore food chain using any of a wide class of popularly accepted and experimentally tested models, we find that the dominant predicted

correlation is between the plant biomass and carnivore abundance, neither of which may show a pronounced statistical relationship with the intervening herbivore population.

System Identification

System identification is a way of considering a wide range of plausible model structures to see which do not appear to represent the system correctly. It can be viewed as both a compromise and a generalization: a compromise because you rely both on prior understanding and on existing data, and a generalization becuase you can consider a wide range of structures rather than having either to build a unique structure which "has" to work -- the Procrustean bed approach -- or use a set of arbitrary statistical forms which may make no biological sense.

Consider the set of all possible models we might construct to describe a fisheries ecosystem. Some we disregard at the outset on basic biological grounds -- they violate conservation laws, etc. Other theories are simply too outlandish, such as the theory that horizontal nutrient transport is due to the fact that "pigs has wings" -- we can ignore that one. I suspect that most linear models could be excluded at this point too.

That leaves us with a large set, possibly disjoint, of possible model formulations. This is very different from the engineering approach which would require that we specify a small cluster of models having a single structure and a reasonable range of different parameter values at this point.

The next step -- this is what is normally referred to as system identification -- is to see which of these possible models are consistent with the data. Different models of course lead to different predictions, the degree of difference depending in part on how much latitude we leave for parameter adjustment. However, we can also find that quite different types of models can lead to indistinguishable results.

The Uniqueness Problem

We have all encountered the idea that different statistical formulations can lead to different model structures -- even plain old multiple regression can generate a multitude of alternate models depending on the method used (Draper and Smith 1981). As Gabriel and Murawski (1985) show, the structure one arrives at through system identification techniques can be sensitive to relatively arbitrary methodological considerations, such as the choice of distance measure in cluster analysis. In fisheries ecology we often end up with the identification of alternate models which are very different. One of the classic cases concerns a situation in which we find a strong inverse correlation between landings of two fish species. A biologist may interpret this as evidence for competition or some similar biological interaction between the species, while an economist might feel that since fishing effort is limited, fishermen simply switch to the species most in demand.

Expressed in formal terms like this, the idea that several models can make biological sense and fit the data is not unreasonable. We are used to having alternate hypotheses, and system identification is in some ways just a more formal way to formulate alternate hypotheses. From a psychological and philosophical point of view however it is not always easy to deal with this sort of ambiguity. Having two or more equally good and substantially different models to deal with creates a degree of tension which can be beneficial -- the philosophers of science like to argue that science spurts ahead through the resolution of conflicts between competing theories -- but this sort of Hegelian dialectic becomes very frustrating if it is not resolved promptly. As Halliday and Pinhorn (1985) point out, fisheries managers are notably unsympathetic towards the inability of scientists to discriminate between conflicting hypotheses.

The general feeling seems to be that if one has alternate hypotheses one has to be able to design a critical experiment to distinguish between them. This is not always easy, and the experimental difficulties associated with fisheries ecology often rule out any quick resolution. During the past year we have heard calls for incredibly large-scale world-wide experiments designed to resolve relatively elementary and long-known problems about fish recruitment. In frustration we often resort to Occam's razor to resolve the issue, but that always leaves us with a nagging feeling that perhaps we are overlooking something important -- just because we cannot prove that something is happening doesn't mean that it isn't, and basing fisheries management on the simplest theory around may not always lead to the best results.

Discussion

This is not the first workshop on interactions in fisneries, and it is unlikely to be the last. The reason is in large part our discomfort about the growing numbers of acceptable models prevalent in the fisheries literature. Even in cases where many fisheries scientists feel certain that there must be an interaction -- in the relationship between cod and capelin along the Newfoundland and Labrador coast for example -- it has been frustratingly difficult to demonstrate it quantitatively, as discussed by Halliday and Pinhorn (1985). I am certain that the reluctance of many scientists and managers to use holistic, as opposed to single-species, models is lack of clear-cut evidence that the holistic models are "right" and single-species models are "wrong". In practice we walk a middle ground -- we don't use the holistic models for quantitative estimates, but we try not to do anything too radically at variance with their implications, such as wiping out what may be important prey species.

I think that we have to learn to recognize that falsifying ecological theories is a difficult and time-consuming process, and we may not have time to find the one true theory before some fairly major commitments about fisheries management have to be made. System identification provides a powerful set of tools for developing models, but one of its strengths is that it generates lots of good models -- it has to, in order not to miss the one that may prove correct in the end. If we are to benefit from these tools,we have to develop strategies for using suites of several

models in the management process, rather than arbitrarily choosing one and ignoring the others.

You are all familiar with the story of Cinderella. You recall that the Prince devoted a major portion of the resources of the Kingdom to identifying the perfect model (wife). I am sure that he would have saved everyone a great deal of time and effort if he had been willing to settle for the first candidate. After all, the alternatives are generally categorized as both simple and straightforward. But would that have been the right choice? Would they have lived happily forever after? Would the children of today's fishermen still like the story?

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Discussion Period

Evans: You mentioned that in three-level trophic models the correlation is between the first and third levels. Is that the correlation of equilibrium levels or of transient responses?

Silvert: I was referring to equilibrium levels. Transient responses depend very much on the time scale. Normally in aquatic systems you assume that the turnover rate decreases as you move toward higher trophic levels.

Evans: Therefore, if Lilly was looking for correlations between predator and prey in a transient state, your criticism that this was an incorrect approach might be inappropriate.

Dickie: It seems to me that there is a consistent theme running through these discussions, which says that the major difficulty in "multispecies" or "holistic" models is that we have to build them to take account of single species problems and that this means we have to build them "from the ground up". Lurking in the background, however, there is an assumption which no one really seems to challenge, which is that there is an upper limit of production which seems to be divided primarily among the species we are going to exploit. It seems to me that there need not be any exclusivity implied by recognizing these two views, the one that there is an overall holistic model which needs to be broken into its parts, and another that we must construct models to take into account a lot of detailed information. That is, we are not concerned with conflicting models that represent different views of the truth, so much as we are interested in defining models that are useful for certain applications. For example, if we are really interested in long-term prediction it would be useful to consider the aggregation of all species caught annually over a 20-year period. It would probably be quite stable. But for most applications we are saying that within this picture it would still be necessary to consider trends of particular species and interpretation of year-to-year changes, possibly just what we do now.

We need to find a way to discuss within the framework of "multispecies" the kinds of model structure that are most useful for meeting certain objectives. We are agreed that predicting annual catch is one such objective. I feel that "reducing risk" in fishing is another. The workshop in some way has to concern itself with balancing the large research effort necessary in order to reduce the level of short-term risks (e.g., uncertainty of next year's catch by lone-liners) against the smaller amount of effort which may be required if we were to try to predict for large scale operations (availability of resources on the Scotian Shelf for the next five years). The desirability of reduction of risks at various levels would offer a criterion for the techniques of measurement which need to be used and the kind and complexity of models which are acceptable for interpreting them.

Silvert: I certainly have no quarrel with that. What you are saying, in relation to my talk, is that several of the possible models which are identified can be rejected because they are not useful, while others can be used for different purposes.

Evans: I hope that we are not going to regard holistic and multispecies as synonomous.

Silvert: I would view miltispecies as a subset of holistic models.

Evans: I disagree, one can build defiantly reductionistic multispecies models.

Silvert: That is true. I stand corrected.

Murawski: This afternoon I have not heard the phrase 'top down modelling' at all. I expected it to come up considering the pragmatic discussion this morning which set the stage for what is essentially a top down analysis of fishery systems. I also think that much of model identification is starting to come around to a top down philosophy where we have an output signal and want to trace it back through a black box.

Silvert: You are quite right. The process of system identification involves a number of procedures which can be described as top down. Top down modelling was discussed at the 1979 St. John's workshop (Can. Spec. Publ. Fish. Aquat. Sci.

59, 1982), and perhaps we should have discussed it more fully this time as well.

Murawski: The obvious conclusion is that managers are not going to buy bottom up philosophy models because there are a lot of parameters that they have no intuitive feeling for in a lot of those models. If models are based on data that they have feelings for we can build on that.

Sinclair: That brings to mind a point by Bob O'Boyle this morning, which is that we seem to be operating under the misconception that our objective is to conserve the resource and maximize social and other benefits. Yet whenever the managers are faced with the reality that a quota is exceeded the first thing they do is allocate more fish. In the squid fishery when fishing days were exceeded they increased the allowance.

Silvert: Is the problem that scientists are ignorant of management, or do they subscribe to a separation of function that not even the managers want? I have found in the past that the scientists want to provide advice but not to become involved in management. I suspect that many scientists do not feel comfortable looking at the management side of things.

Atkinson: To carry this further, one reason that the scientists wish to be dissociated from management is that managers will often make political decisions that are in direct conflict with biological advice.

Dickie: Isn't it a matter of time scales? It seems that biologists have got themselves locked into looking at particular time scales. There were two remarks today that interested me. One was by Steve Murawski who showed cluster analysis of years which seemed to fall together. This suggests that there are patterns of interactions among species which persist for periods of time and yet, almost all the analyses we look at tend to be year by year. Perhaps we are not looking at an appropriate time scale for a multispecies pattern. The other point was referred to by Al Pinhorn this morning which is that the groundfish are different from the pelagics, an observation which I have been hearing for years. Is this telling us something about temporal and spatial patterns which are important to the type of multispecies generalizations we wish to discuss in this workshop?

Murawski: I agree that system identification is a three dimensional process, with time as one of the dimensions.

Silvert: Some people have attempted to look at long time scales, for example using the fossil record. I believe these people are often viewed as being on the fringes.

SESSION IV:

THE SCOTIAN SHELF EXPERIENCE

Fish Distributional Patterns on the Continental Shelf and Slope From Cape Hatteras to the Hudson Strait -- A Trawl's Eye View

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This is a preliminary exploratory analysis of large scale patterns of fish distribution on the northeast coast of North America. Data from about 13,000 groundfish survey trawl sets made between 1970 and 1980 were aggregated into two depth zones in 91 bands along the coast. The depth zones were 50-200 m and greater than 200 m. The bands were 30 nautical miles wide approximately normal to the shelf edge. Within each depth zone bands were clustered on the basis of presence or absence of 85 taxa of fishes using the Bray-Curtis distance index and Ward's clustering method. Similar results were obtained in both depth zones. Major breaks occurred at the tail of the Grand Banks of Newfoundland, in the middle of the Scotian Shelf, and in the area of Nantucket Shoals. At lower levels of dissimilarity, adjacent groups of bands again clustered out and tended to correspond to topographic and hydrographic features in the region.

Introduction

The objective of this preliminary exploratory analysis is to look for large scale patterns in fish distribution on the east coast of North America and to consider their relationship to the topography and hydrography of the shelf and slope.

The underlying concept is that discontinuities in faunal distribution might relate to functional discontinuities between communities or ecosystems and thus provide some insight as to where to place boundaries for management purposes, or if indeed this is even a reasonable thing to do.

Our approach to pattern seeking is to use cluster analysis of fish species presence/absence data aggregated in bands approximately normal to the Shelf edge.

Methods

The basic data for this analysis were tow-by-tow catches for all U.S. and Canadian groundfish trawl surveys carried out between Cape Hatteras and Hudson Strait in the period 1970-1980 (Doubleday 1981). Only species with 100 or more occurrences in the entire data set were considered sufficiently abundant to be worth including in the analysis. This gave approximately 85 taxa of fishes (Table 1).

The continental shelf from Cape Hatteras to the tail of the Grand Banks of Newfoundland was divided up into 85 bands each 30 nautical miles wide, lying approximately normal to the shelf edge. In the region of Georges Bank, each band was divided into an offshore, Georges Bank segment, and an inshore, Gulf of Maine segment (Figure 1). This division was made in view of the topographic distinctness of these two regions and the deep water which separates them. From the tail of the Grand Banks north to Cape Chidley the bands were parallel to the lines of latitude at 30' intervals.

The species data were then aggregated to give frequency of occurrence of each species in each band for each of two depth zones; 50-200 m and greater than 200 m.

The occurrence matrices of species in bands were clustered using CLUSTAN (Wishart 1978). The distances among bands and among species were estimated with the Bray Curtis index:

$$D_{ij} = \frac{(S \text{ in } i \text{ but not } j) + (S \text{ in } j \text{ but not } i)}{(2 \times (\text{total } S \text{ in } i \text{ and } j))}$$

where S = number of species.

To produce dendrograms, the distance matrices were sorted using Ward's "method which combines the two clusters whose fusion yields the least increase in the error sum of squares" (Wishart 1978). The error sum of squares is the sum of the distances from each individual to its parent cluster.

Results

The number of tows was not uniform across bands, particularly in the shallow depth zone where it peaked at about 300 tows then declined to the north to about 10 tows/band (Figure 2).

Furthermore, within each depth zone the average depth-per-tow tended to increase in a northerly direction (Figure 3). Consequently, it may be desirable to consider narrower depth zones, but in northern bands the number of tows per band is already low.

In deep water, species richness remains consistently high, relative to the shallow zone, at from 8-12 species-per-tow (Figure 4). In depths of 50-200 m the average number of species-per-tow is highest across Georges Bank then declines steadily to just south of Flemish Cap where it increases rapidly over 5-6 bands then tails off again to the north (Figure 4). The rapid increase in species richness corresponds to an increase in average depth per tow (Figure 3). The interactions between depth, latitude and species richness remain to be sorted out.

In Figures 5 and 6 are some typical species distributions in relation to the south to north gradient of bands. Butterfish and offshore hake are truncated to the south, Greenland halibut and northern wolffish to the north. Thorny skate, redfish (Sebastes spp.) and common grenadier (marlinspike) are widely distributed in the area covered by the study. The types of distributions seen are consistent with the expected patterns based on geographical ecological theory (MacArthur 1972).

Dendrograms show the clusters of bands in each depth zone (Figures 7 and 8). A cleaner result was obtained for the shallow zone than for the deep zone. In the former the clusters of bands are for the most part geographically contiguous.

For each of the two depth zones there is a two-way table of species occurrence in bands (Figures 9 and 10) with bands ordered in the same sequence as in the dendrograms and species ordered in the sequence generated by a clustering of species based on their presence/absence in bands.

Discussion

Although the analysis has been undertaken using the two depth intervals 50 m - 200 m and 200 m, it should be emphasised that the data are derived from bottom trawl surveys which are restricted to trawlable bottom. This means that over much of the area, inshore data is lacking and the reported ranges of species occurrance will sometimes appear rather odd (e.g. - cunner, which is commonly caught in the harbours of the southern part of the east coast of Newfoundland does not occur in this offshore data set north of Band 23 (Browns Bank)).

Though the analysis was undertaken for both the clustering of bands on species presence and the clustering of species on presence in bands, it is the former that contains most interest from the point of view of this workshop as we attempt to examine the distribution of fish relative to their environment. Thus, we will focus this discussion on the former analysis and consider only the major clusters. The number of major clusters seen in each dendrogram will depend on the level of dissimilarity considered. The level chosen can be quite arbitrary. In this case levels of 2.5-5.5 will result in 3 or 4 clusters in each analysis. We consider these to be distinct and will restrict our interpretaton to them.

For the 50-200 m depth zone (Figures 7 and 11) the major groupings of bands are:

a) A southern group extending from Cape Hatteras to Cape cod.

b) A southern central group extending from Cape Cod to the Scotian Gulf in the middle of the Scotian Shelf, including the Gulf of Maine and Georges Bank (ending at Band 24).

c) A central group extending through the remainder of the Scotian Shelf across the Laurentian Channel and down the eastern side of this channel to the Tail of the Grand Bank.

d) A northern group extending from the Tail of the Grand Bank northward Cape to Chidley.

At the three group level, groups a and b would be combined.

In the depth zone greater than 200 m the four major groups (Figures 8 and 12) are:

e) A southern group extending from Cape Hatteras to Cape Cod.

f) A south central group extending from Cape Cod to Sable Island and including the Gulf of Maine.

g) A north central group extending from Sable Island to the Laurentian Channel and down the eastern side of the channel to the Tail of the Grand Bank.

h) A northern group extending from the Tail of the Bank northward to Cape Chidley.

In this case the aggregation to three major groups would combine f and g. Clearly, at the four group level the results of the analyses are very similar for both depth zones, and we will discuss them together.

A fundamental conclusion, indicated by both analyses, is that, on the basis of species occurrance over some 24° of latitude, the major fish faunal boundaries do not coincide with deep water. Some of the deep channels e.g. - the Laurentian Channel and the Northeast Channel between Georges and Browns Banks, which one might intuitively think of as major boundaries, at least for the shallow water species, are relatively unimportance in this respect. Most of the following interpretation of the relationship of clusters to oceanographic conditions is based on features which would be most influential in the 50-200 m depth zone. The close correspondence between the results in the two depth zones is a question requiring further consideraton. It could mean that the influence of the oceanographic

features is felt at greater depths than we anticipate, or that there are other correlated factors which we have not considered.

In both the analyses a major boundary between clusters shows up at the Tail of the Grand Bank. In the 50-200 m analysis the northern group (Tail of the Grand Bank to Cape Chidley) appears more similar to the central group, whereas at depths greter than 200 m this northern group appears well separated from the other three groups.

Examination of the two way tables in Figures 10 and 11 and the use of some imagination provides some insight into why the bands clustered in the way that they did. Attention is drawn to the block pattern, the termination of many of the warm water species in the area of the Trail of the Grand Banks (angler, silver hake, argentine, long finned hake, etc.) and the limited distribution of some northern species south and west of this point (rough headed grenadier, round nosed grenadier (rock grenadier)).

It is not difficult to understand why a barrier for the shallow water species exists at the Tail of the Bank. The whole area from Northern Labrador through to the Eastern edge of the Grand Bank is influenced by the cold Labrador Current, whereas the southwest edge of the Grand Bank is much warmer, in general being influenced by incursions of slope water over the edge of the bank. In the deeper water, however, below the influence of Labrador Current, and where the warmer water of Atlantic origin provides relatively stable temperature conditions from Sable Island to Northern Labrador (albeit there is a small gradual decrease of 1° or 2° C northward and the cold intermediate layer extends deeper in the north), it is more difficult to understand the distributional break. However, the oceanographic regime on the southwest edge of the Grand Bank is certainly more variable than that on the Southeast edge of the Grand Bank.

Southwards, the next major division between clusters in both depth groups occurs in the middle of the Scotian Shelf. At depths 50-200 m the break is to the southwest of the Scotian Gulf, whereas in the analysis of depths of greater than 200 m it is in the vicinity of Sable Island. This is consistent with finer scale analyses of the Scotian Shelf by Mahon et al. (1984) and Mahon (1985) which show the biological and environmental basis for this mid-shelf break. Basically, incursions of warm saline slope water in the region of the Scotian Gulf change the hydrological characteristics of the water mass as it moves south and west along the shelf (Hachey 1961; McLellan 1954a, 1954b, 1955; Smith et al. 1978; Trites 1982).

Again in both cases, the next major break occurs at Cape Cod. Taylor, Bigelow and Graham (1957) have pointed out the difference in qualitative composition of the fish fauna from north to south around Cape Cod in winter, and it is not surprising that the major break between clusters within Area A occurred at the Marthas Vinyard-Nantucket Shoals area. Several authors have noted a rather abrupt division between temperatures to the east and west of Cape Cod (Bigelow 1933; Parr 1933; Davis 1979). Parr (1933), in particular, refers to the cold water barrier which develops in the Cape Cod-Nantucket Shoals area during the summer months and quotes biological evidence from the summer distribution of migratory species which clearly point to the existance of an effective temperature barrier in the region of Cape Cod.

The main differences between the deep and shallow analyses are (a) the exact position of the mid-shelf break discussed above and (b) the way in which the groups at the four group level combine at the three group level. In the deeper water, the mid-Scotian Shelf break is deemphasized at the three group level, whereas, at shallower depths, the Cape Cod break is deemphasized. Considering the coarse level of spatial aggregation and the preliminary nature of this study we are reluctant to pursue the interpretation of these differences.

Acknowledgements

We would like to thank Dr. M. Grosslein of the US NMFS NE Fisheries Center for making the US survey data available to us for this preliminary analysis. Also, thanks to Mr. R. O'Boyle, and Mr. T. MacEachern for their assistance in processing the volumes of data.

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Table 1. Species and codes used in the analysis.

Code	Common Name (Scientific Name)
ALEWIF	Alewife (Alosa pseudoharengus)
ALIGAT	Alligatorfish (Aspidophoroides spp.)
AMMDUB	Northern sandlance (Ammodytes dubius)
AMPLAC	American plaice (Hippoglossoides platessoides)
AMSHAD	American shad (Alosa sapidissima)
ANGLER	Angler (Lophius americanus)
ARCCOD	Arctic cod (Boregoadus saida)
ARCHOK	Arctic hookear sculpin (Artediellus uncinatus)
ARGENT	Argentine (Argentina silus)
ARMROB	Armored sea robin (Peristedion miniatum)
ARPOUT	Arctic eelpout (Lycodes reticulatus)
ATLPCH	Atlantic sea poacher (Agonus decagonus)
ATWOLF	Atlantic wolffish (Anarhichas lupus)
BARCUD	Barracudina (Paralepis spp.)
BARSKT	Barndoor skate (Raja laevis)
BLFISH	Bluefish (Pomatomus saltatrix)
BLKDOG	Black dogfish (Centroscyllium fabricii)
BLUBAK	Blueback herring (Alosa aestivalis)
BRISKT	Brier skate (Raja eglanteria)
BUTTER	Butterfish (Peprilus triacanthus)
CAPLIN	Capelin (Mallotus villosus)
CODFIS	Cod (Gadus morua)
CONGER	Conger eel (Conger oceanicus)
CUNNER	Cunner (Tautogolabrus adspersus)
CUSKFS	Cusk (Brosme brosme)
DBSHAN	Daubed shanny (Luropenus maculatus)
FAWNEL	Fawn cusk eel (Lepophidium cervinum)
FBROCK	Fourbeard rockling (Enchelyopus cimbrius)

Table 1. Continued

Code	Common Name (Scientific Name)
FILFIS	Planehead filefish (Monacanthus hispidus)
FRSPTF	Fourspot flounder (Paralichthys oblongus)
GRAYEL	Gray's cutthroat eel (Synophobranchus kaupi)
GREENH	Greenland halibut (Reinhardtius hippoglossoides)
GRNEYE	Shortnose greeneye (Chlorophthalmus agassizi)
GULFFL	Gulf Stream flounder (Citharichthys arctifrons)
HADDOK	Haddock (Melanogrammus aegelfinus)
HALBUT	Halibut (Hippoglossus hippoglossus)
HERING	Herring (Clupea harengus)
LFINHK	Longfin hake (Urophycis chesteri)
LHSCUL	Longhorn sculpin (Myoxocephalus octodecemspinosus)
LITSKT	Little skate (Raja erinacea)
LUMFIS	Lumpfish (Cyclopterus lumpus)
MAKREL	Mackerel (Scomber scombrus)
MARLIN	Common grenadier (Nezumia bairdi)
MLSCUL	Mailed sculpin (Triglops murrayi)
MULPRL	Muller's pearlside (Maurolicus muelleri)
NORHAG	Northern hagfish (Myxine glutinosa)
NORROB	Northern sea robin (Prionotus carolinus)
NOWOLF	Northern wolffish (Anarhichas denticulatus)
OCPOUT	Ocean pout (Macrozoarces americanus)
OFFHAK	Offshore hake (Merluccius albidus)
POLLOK	Pollock (Pollachius virens)
POSCUL	Polar sculpin (Cottunculus microps)
RASHAN	Radiated shanny (Ulvaria subbifurcata)
REDFIS	Redfish (Sebastes spp.)
REDHAK	Red hake (Urophycis chuss)
RNDHER	Round herring (Etrumeusteres)
ROCKGR	Rock grenadier (Coryphaenoides rupestris)
ROSFIS	Rosefish (Helicolenus dactylopteius)
ROSSKT	Rosette skate (Raja garmani)
RUFHED	Roughhead grenadier (Macrourus berglax)
SANLAN	Sand lance (Ammodytes americanus)
SCUPFS	Scup (Stenotomus chrysops)
SEABAS	Southern seabass (Centropristis striatus)
SEARAV	Searaven (Hemitripterus americanus)
SHPOUT	Shorttail eelpout (Lycodes vahli)
SHSCUL	Shorthorn sculpin (Myoxocephalus scorpius)
SILHAK	Silver hake (Merluccius bilinearis)
SMODOG	Smooth dogfish (Mustelus canis)
SMOSKT	Smooth skate (Raja senta)
SNBLEN	Snake blenny (Lumpenus lumpretaeformis)
SPIDOG	Spiny dogfish (Squalus acanthias)
SPINEL	Spiny eel (Notacanthus nasus)
SPISKT	Spinytail skate (Raja spinicauda)
SPOSKT	Winter skate (Raja ocellata)
SPOTHK	Spotted hake (Urophycis regius)
SPWOLF	Spotted wolffish (Anarhichas minor)
STRROB	Striped sea robin (Prionotus evolans)
SUMMER	Summer flounder (Paralichthys dentatus)
THOSKT	Thorny skate (Raja radiata)
WHIHAK	White hake (Urophysis tenuis)
WINDOW	Windowpane (Scophthalmus aquosus)
WINTER	Winter flounder (Pseudopleuronectes americanus)
WITCHF	Witch flounder (Glyptocephalus cynoglosus)
VACHON	Vachon's eelpout (Lycodes esmarki)
YELLOW	Yellowtail (Limanda ferruginea)

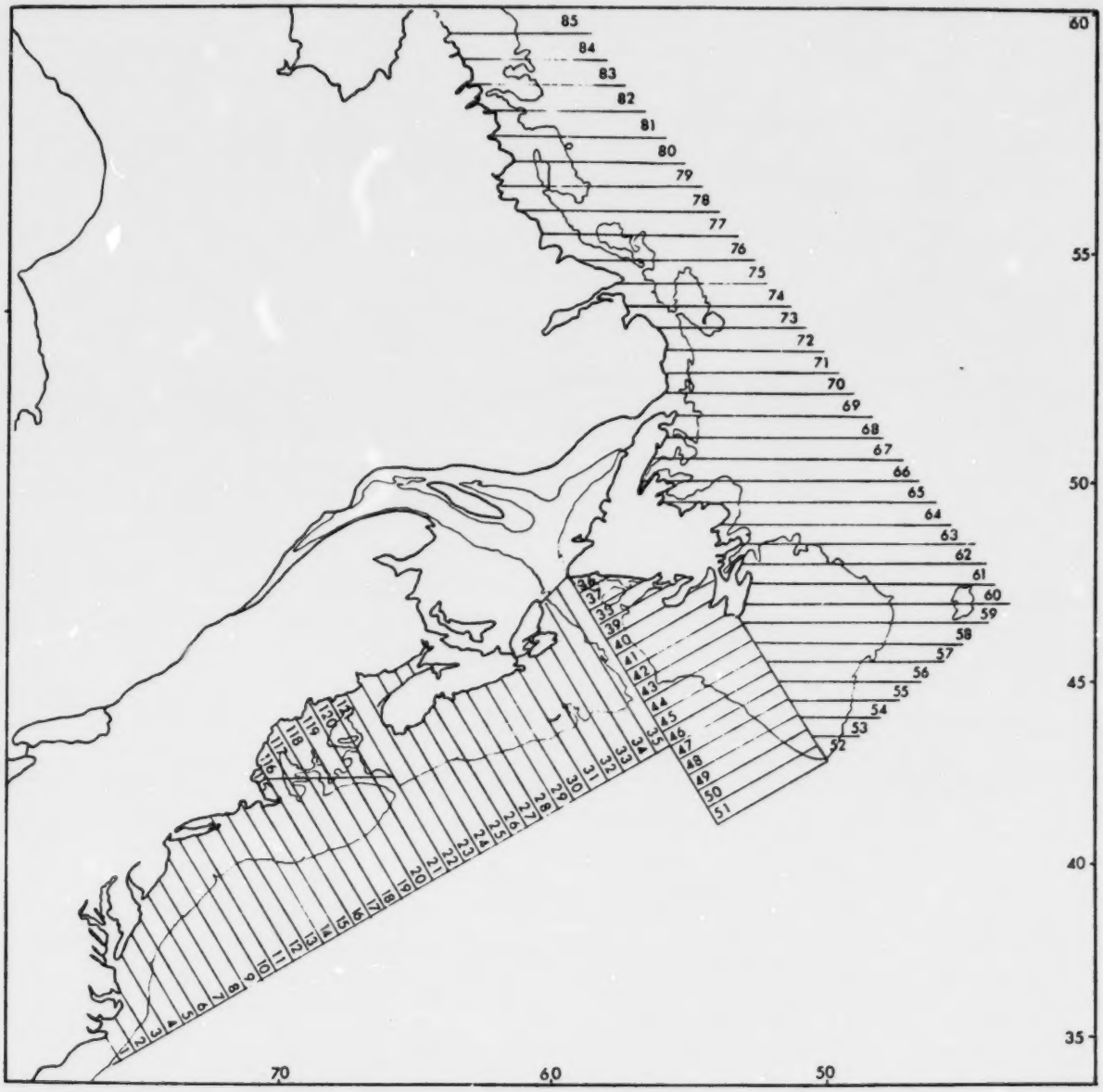


Figure 1. The breakdown of the continental shelf and slope into bands within which sets were aggregated

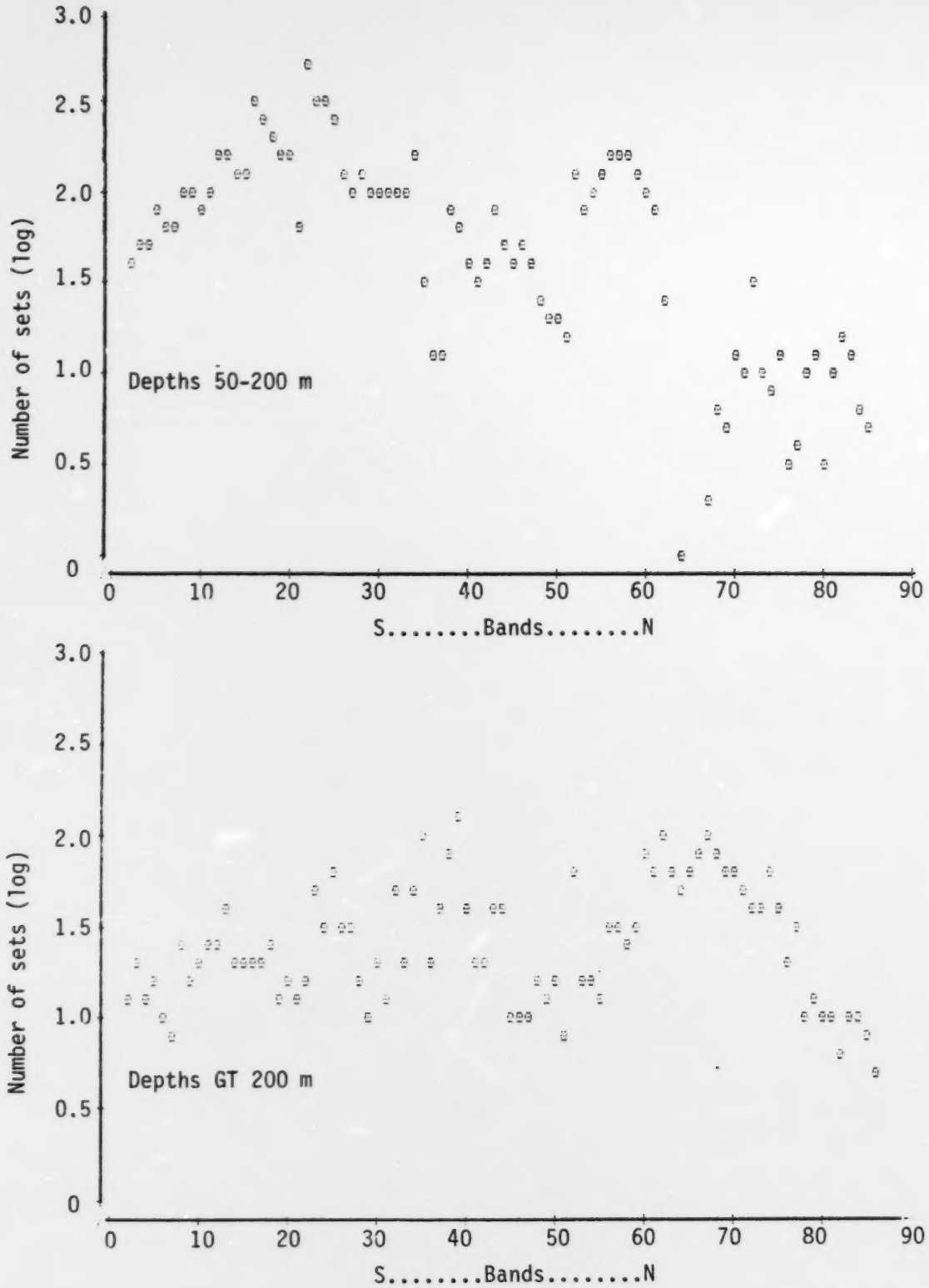
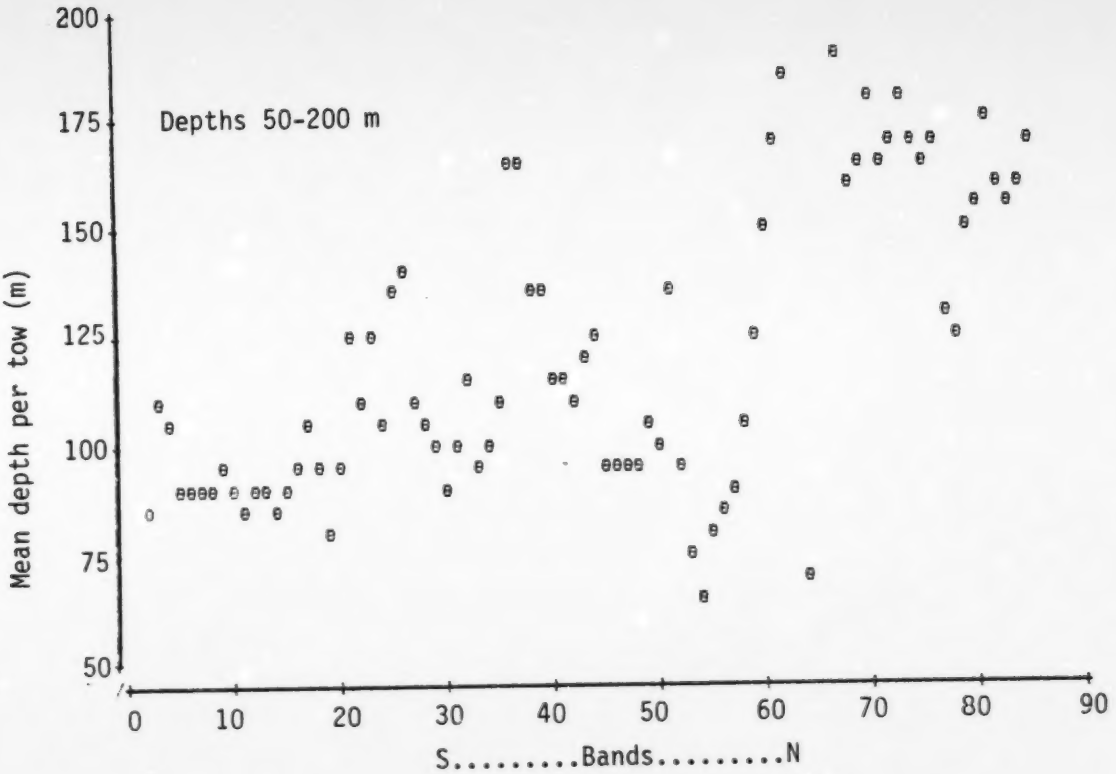


Figure 2. The (log) number of tows per band in each depth zone



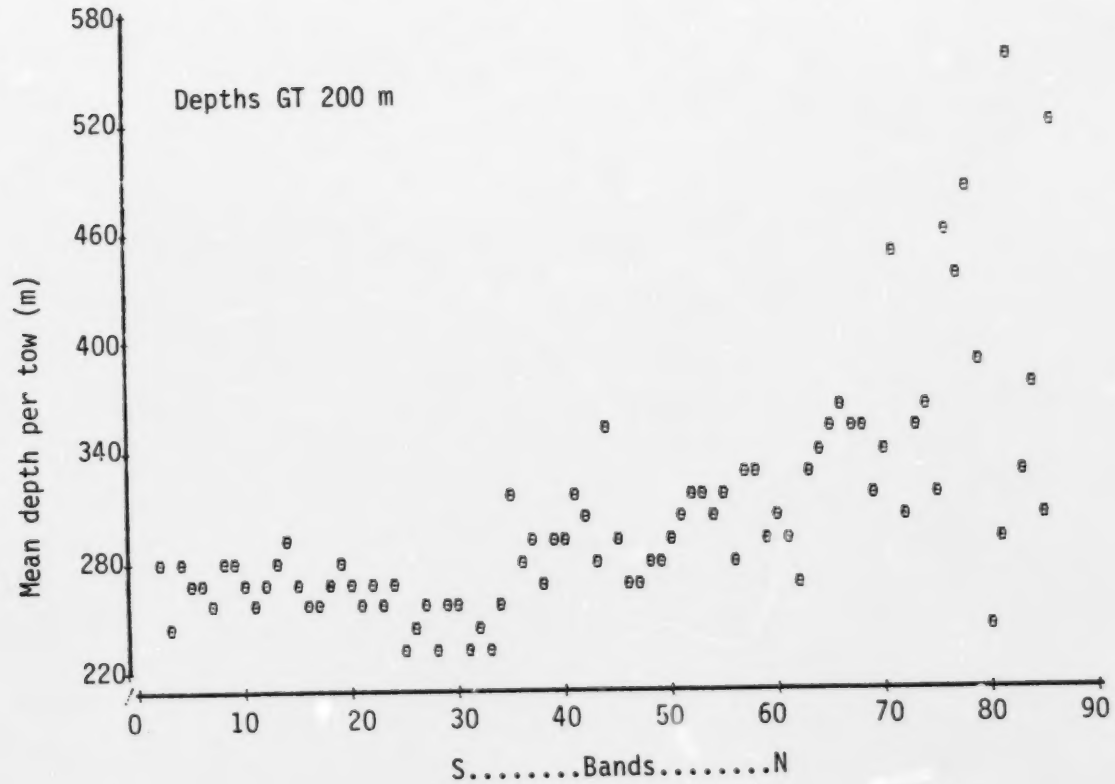
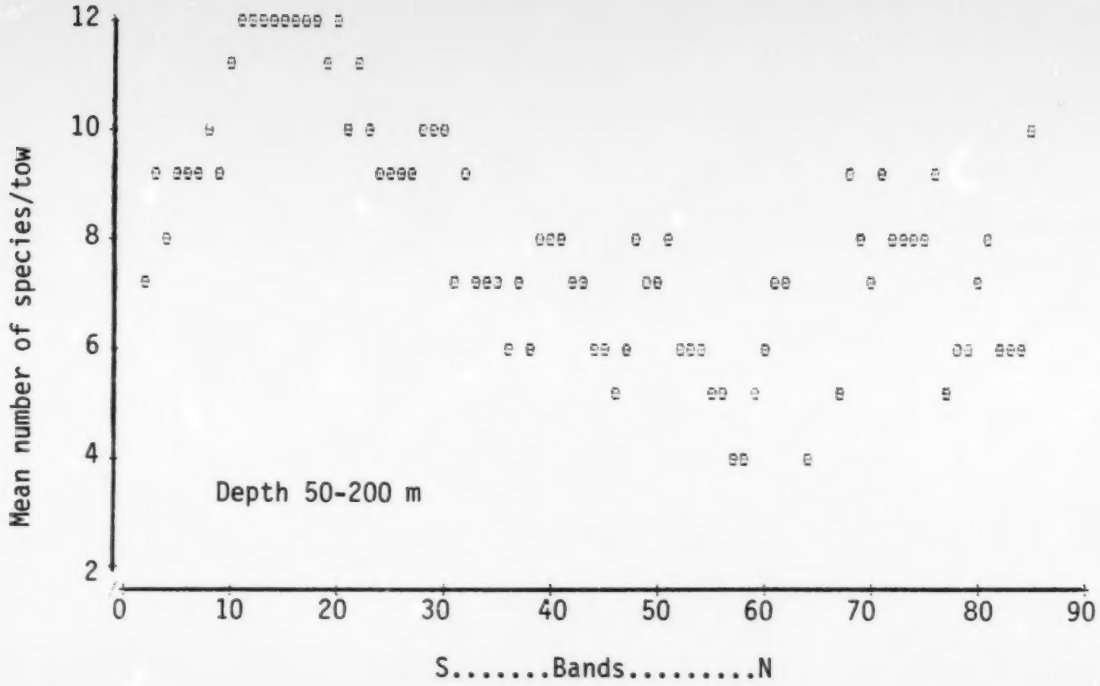


Figure 3. The mean depth per tow in bands in each depth zone



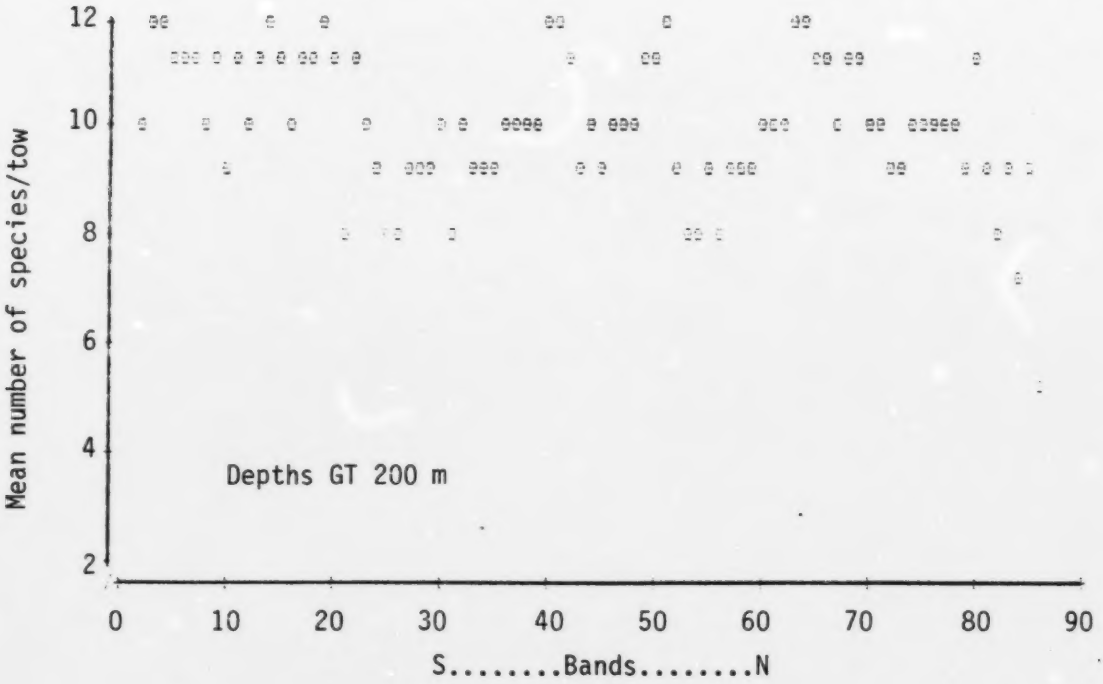


Figure 4. The average number of species per tow in bands in each depth zone

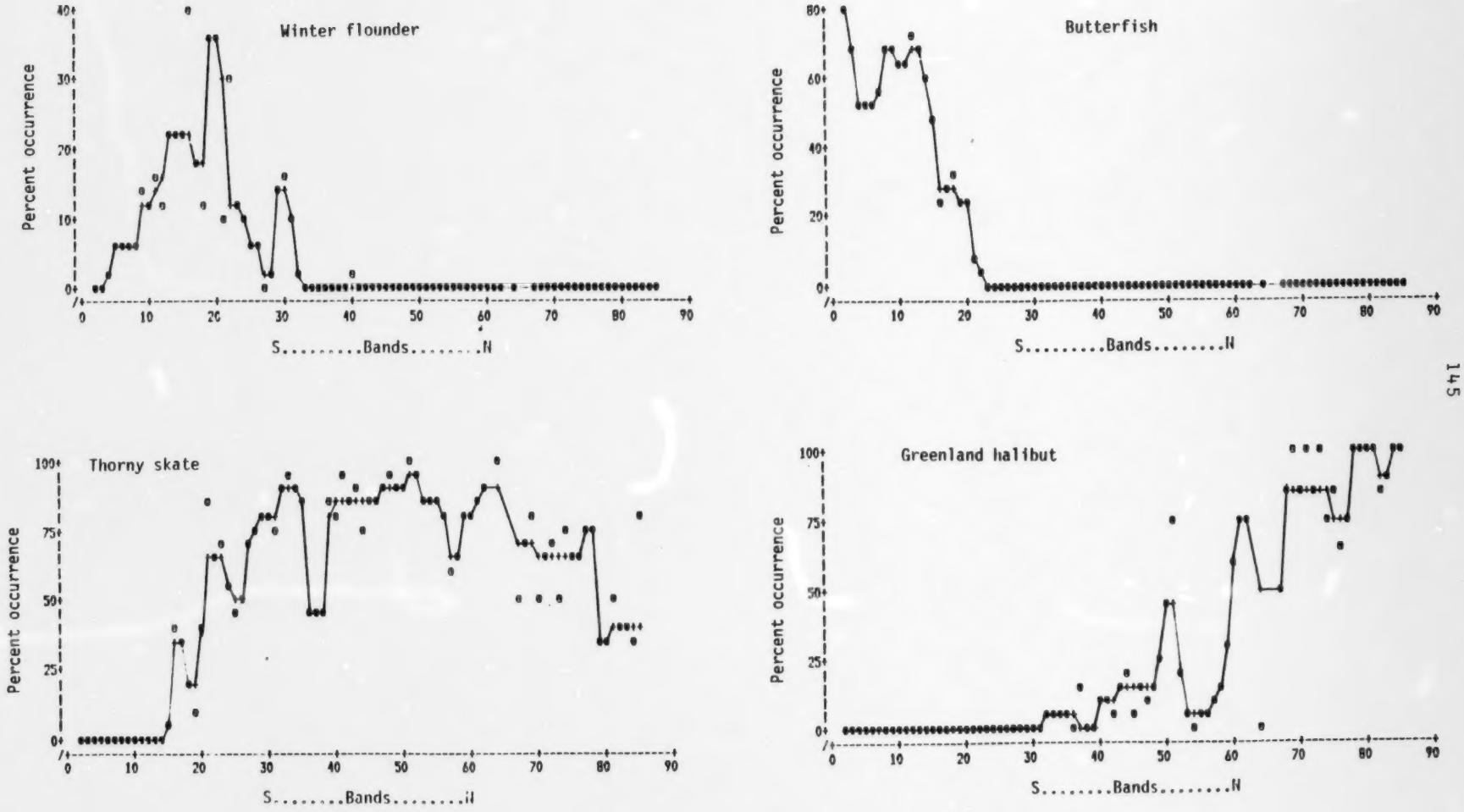


Figure 5. Percent occurrence of selected species in tows in each band (depths 50-200m)

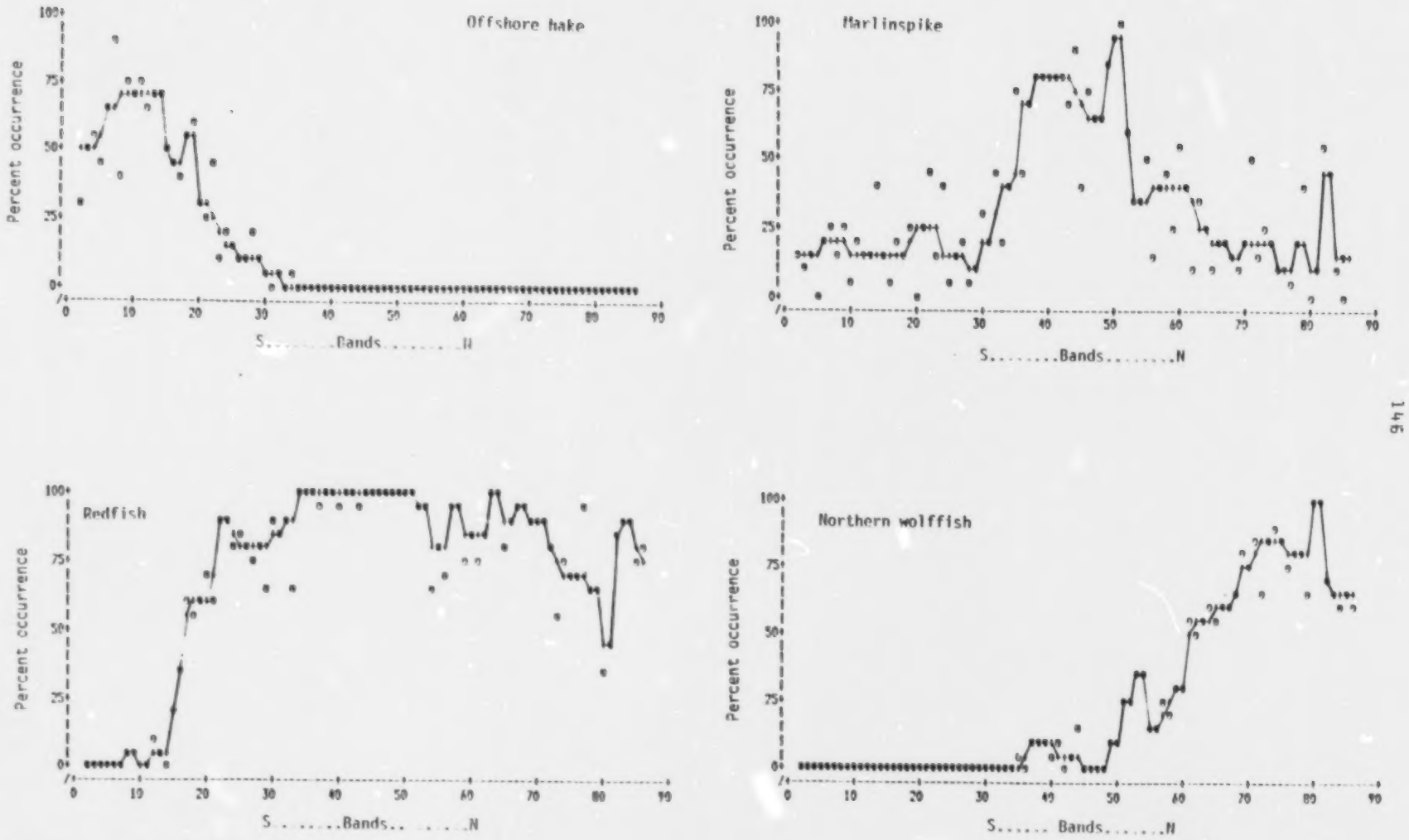


Figure 6. Percent occurrence of selected species in tows in each band (depths greater than 200m)

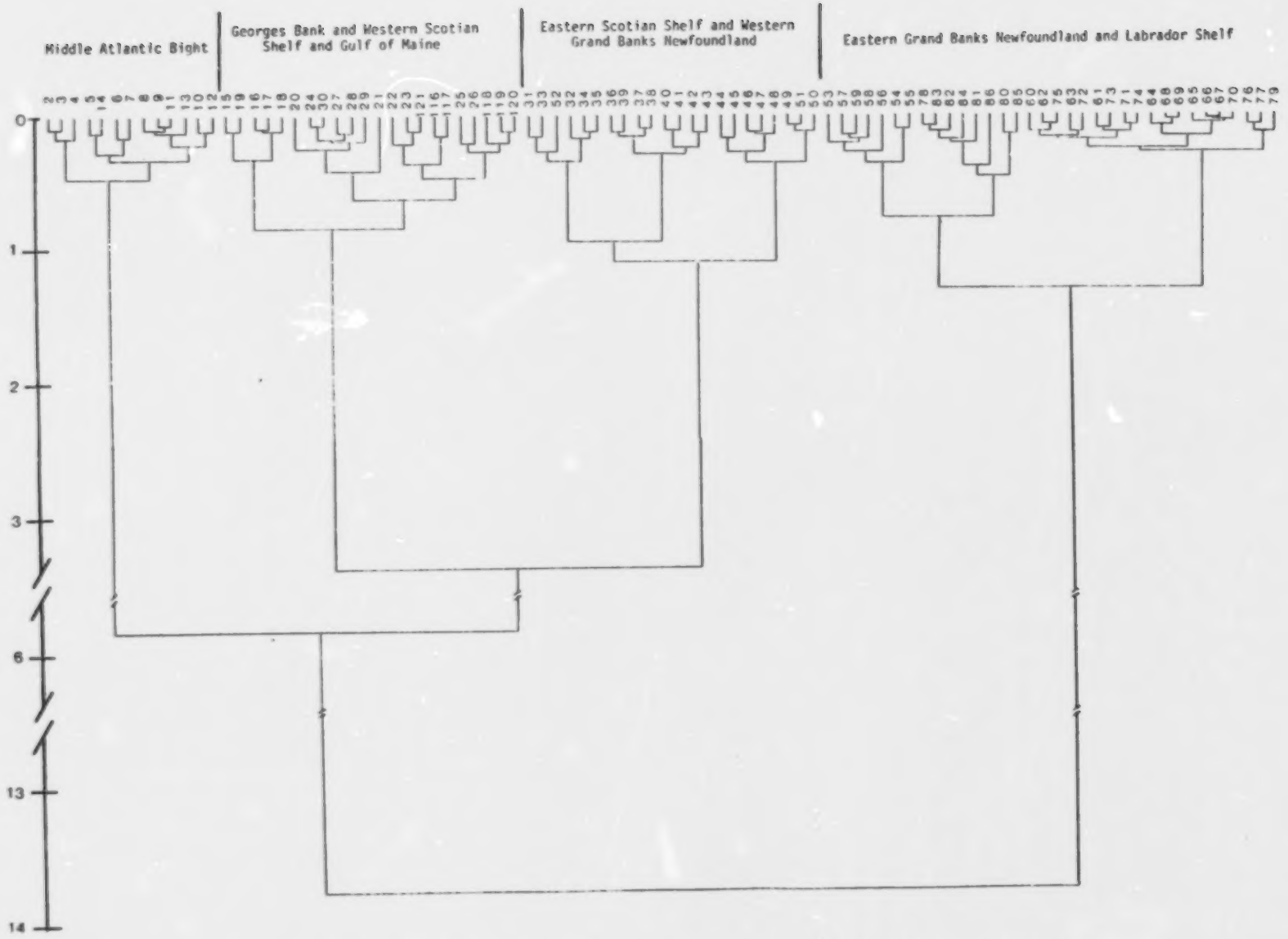


Figure 7. The clustering of bands based on species occurrence at depths of 50-200 m

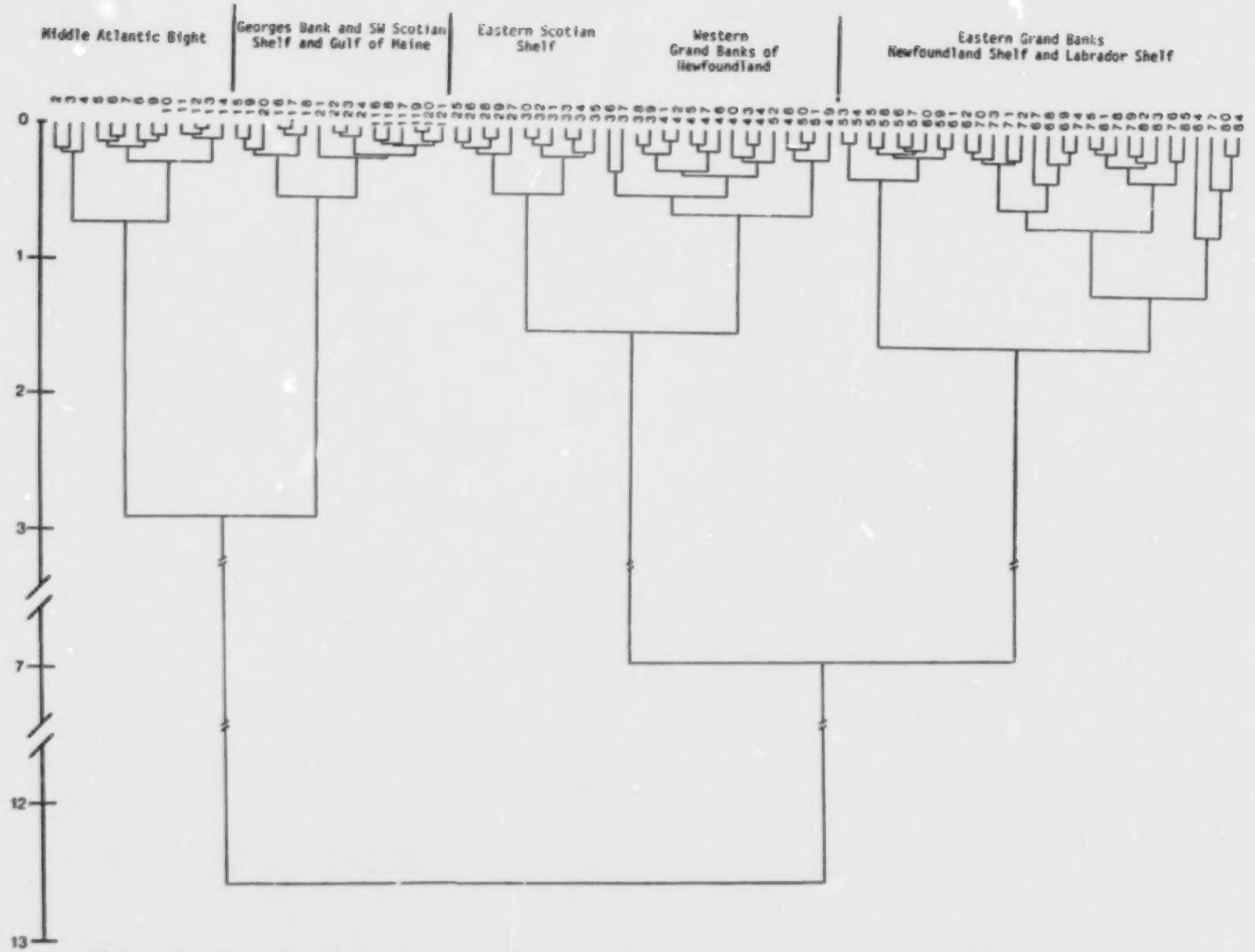


Figure 8. The clustering of bands based on species occurrence at depth greater than 200 m

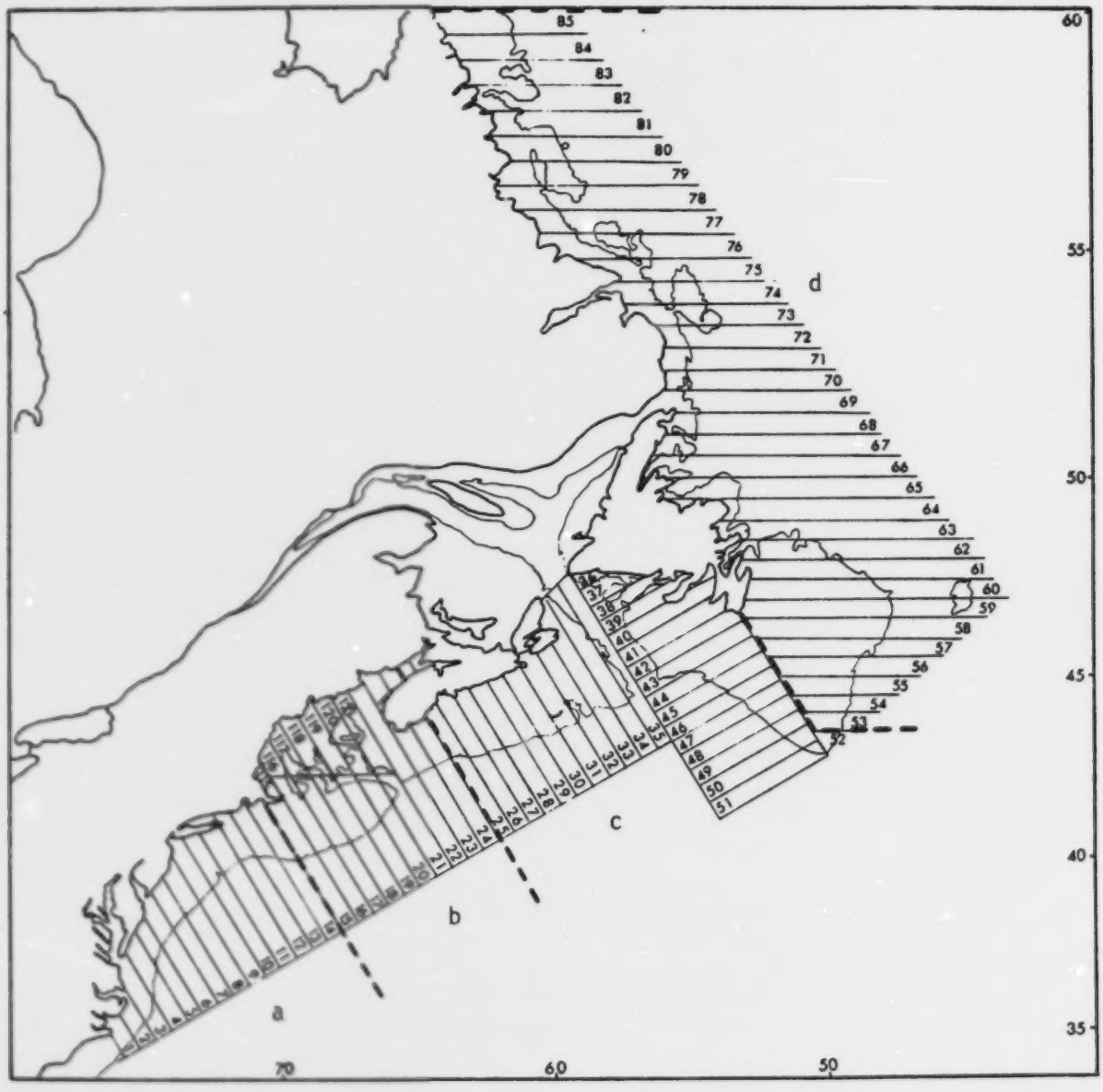


Figure 9. The four major clusters of bands using the occurrence of fishes at depths of 50-200 m

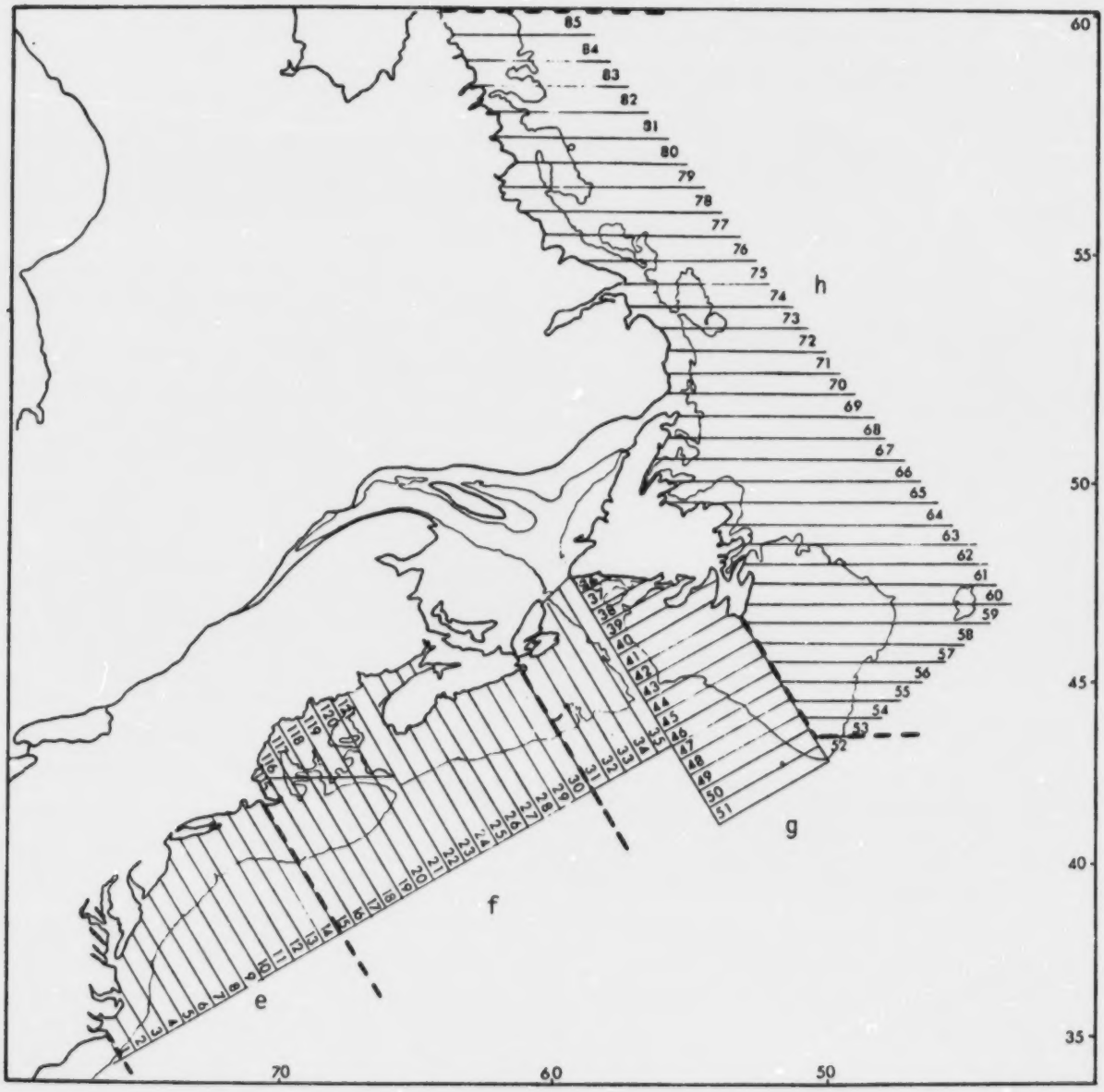


Figure 10. The four major clusters of bands using the occurrence of fishes at depths greater than 200 m

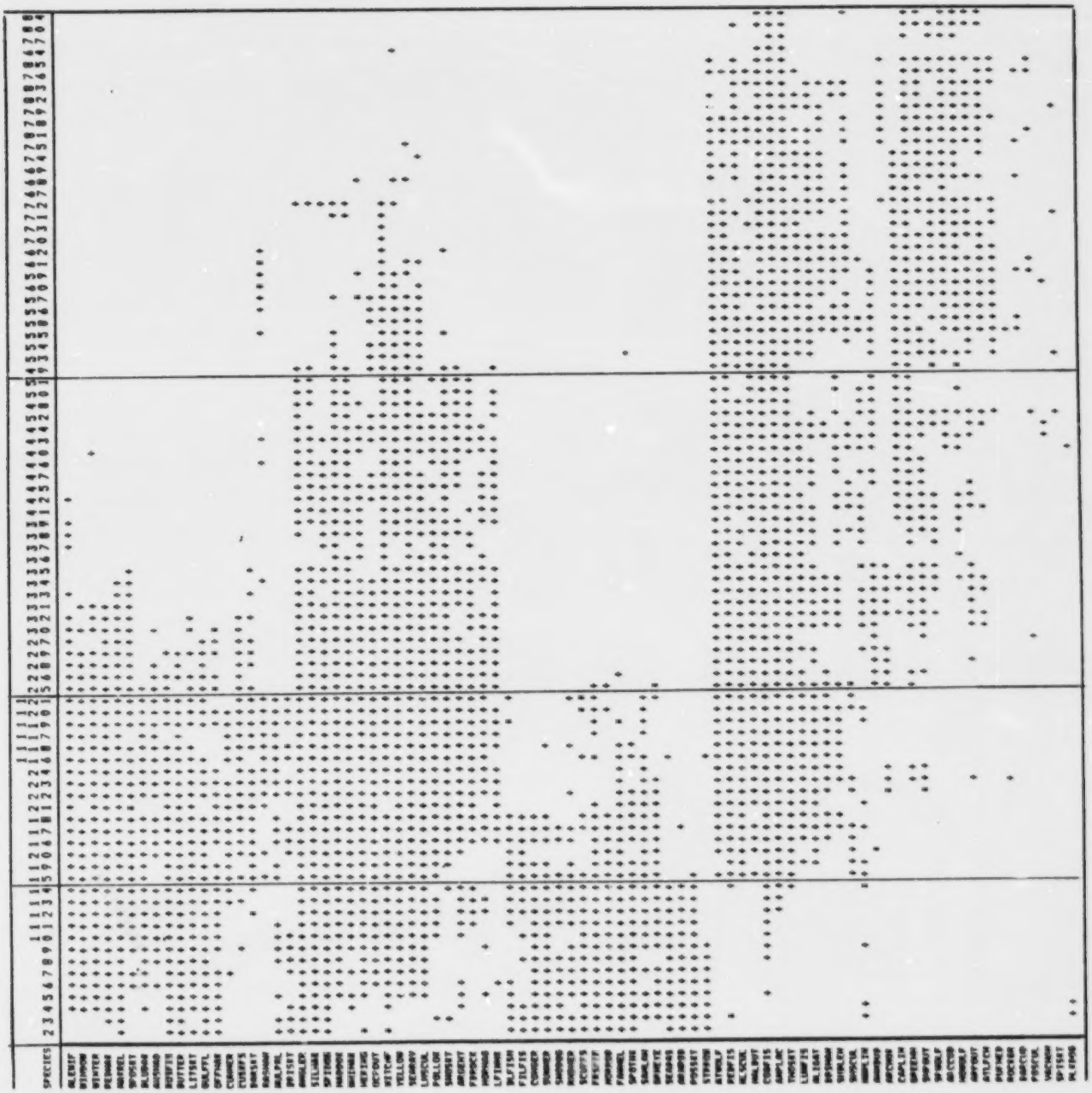


Figure 11. Two-way table of species occurrence in bands at depths of 50-200 m. The bands are in the same order as the corresponding dendrogram, the species are in the order produced by clustering species on their occurrence in bands.

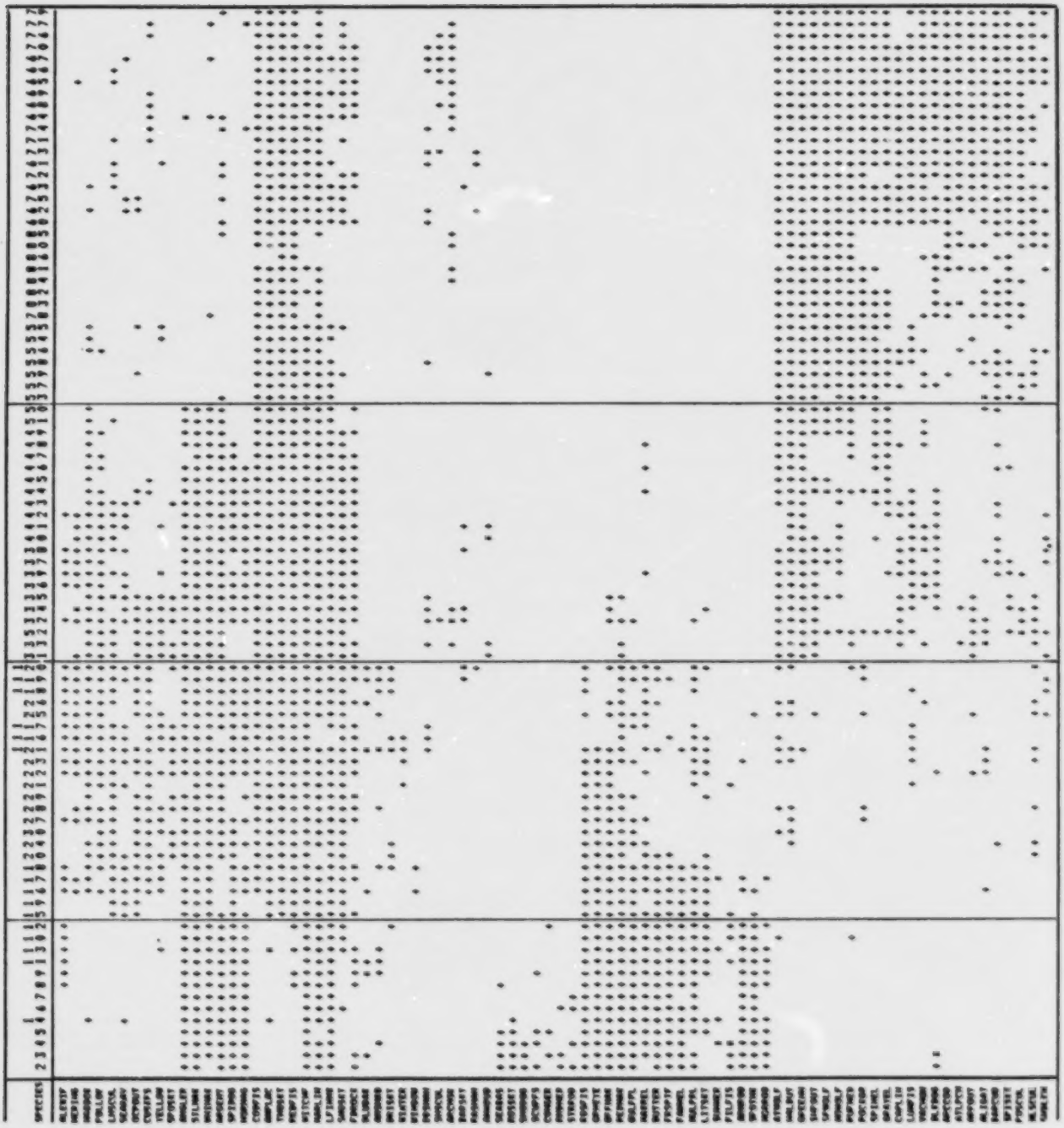


Figure 12. Two-way table of species occurrence in bands at depths greater than 200 m. Bands and species are ordered using the same procedure as in Figure 11

Groundfish Assemblages on the Scotian Shelf

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Mahon, R. 1985. Groundfish Assemblages on the Scotian Shelf. p. 153-162. In: R. Mahon [ed.] Towards the Inclusion of Fishery Interactions in Mangement Advice. Can. Tech. Rep. FTsh. Aquat. Sci. No. 1347.

Cluster and discriminant analyses of summer groundfish trawl surveys (1970-1981) explore the spatial and temporal patterns of groundfish distribution on the Scotian Shelf. This was done with a view to evaluating the applicability of the assemblage region and assemblage production unit approach in this area. Year by year analysis indicated a high degree of consistency in the groupings of sites and species. The suites of environmental variables which best discriminated among site groups were also similar among years. In view of the overall consistency among years, a summary analysis was carried out in which species abundances in sampling strata were averaged over the entire time period. The overall picture was that the Scotian Shelf could be divided into a number of assemblage regions with characteristic species composition. These could be useful in regulating by-catch with a view to optimizing overall catch. However, there was insufficient information on feeding to determine if the species groups defined by the clustering procedure were trophically linked. Based on available information it appears unlikely that they would be relatively discrete trophic units. It is likely, though it remains untested, that environmental preferences determine species distributions moreso than biological interactions.

Introduction

This paper reviews the temporal and spatial patterns of groundfish distribution on the Scotian Shelf described by Mahon et al. (1984), and presents a further summary analysis.

The data on which these studies are based are the research survey catches on the Scotian Shelf and in the Bay of Fundy (Figure 1) for the period 1970- 1981. The techniques used are cluster analysis of sets and species and discriminant analysis of cluster groups of sets using available environmental variables.

The basic concept is that groupings of sites and species which are revealed in this way may be useful as management units (Gabriel and Murawski 1985). This may be in a purely operational sense, as in attempting to deal with technological interactions (Murawski et al. 1983).

Methods

Full details of the methods will not be presented here. For these see Mahon et al. 1984. An overview of the procedures used is as follows:

DATA PREPARATION

Site and species selection matching matrices of:

(a) sites x species
(b) sites x environmental variables

CLUSTER ANALYSIS AND ORDINATON

Matrices of distances between:

(a) sites
(b) species

Dendrograms showing relationships between:

(a) sites
(b) species

Two-way table of sites x species (shaded by abundance).

Groupings of:

(a) sites
(b) species

DISCRIMINANT ANALYSIS

Discrimination of site groups using environmental variables.

DISPLAY OF RESULTS

Mapping of site groups in geographical space.

Mapping of:

(a) sites (groups)
(b) species (groups inenvironmental space (discriminant axes)

The numbers of individuals per standard tow were used in estimating the inter-set and inter-species distances. These were square root transformed to reduce the influence of high values.

The inter-set distances were estimated using the Zero Adjusted Distance index (ZAD). This is based on a Bray Curtis index but corrects for a number of undesirable properties of that index (e.g. ZAD does not become asymptotic when species turnover is complete; ZAD reduces the impact of nonmonotic species distributions in relation to environmental gradients).

The inter-species distances were estimated using the Bray Curtis index with the TWOSTEP modification of Austin and Belbin (1982) and the step-across approach of Williamson (1978). This index measures relative habitat preference rather than overlap for each pair of species. Basically, relative habitat preference is estimated by looking at the way each member of a pair of species relates to all other species in a data set. In contrast overlap is estimated by looking at the way in which the members of the pair relate to each other directly. Therefore, members of a pair may have very similar habitat preference but overlap very little if they occupy similar habitats and replace each other in habitat patches or geographically.

Results

Year By Year

The findings of the individual survey (= year) analyses can be summarized as follows:

1) There was a high degree of consistency among years in the spatial aggregation of sets. Figure 2 summarizes the areas which came out of the clustering of sites year after year.

   Whereas, these areas can be identified in each survey there are changes from year to year in the interrelationships among them. For example, in some years the Sable Island Bank area is most similar to the Bay of Fundy but in others it is most similar to the northeast shelf area.

2) The configuration of environmental variables which resulted from the discriminant analysis of site groups was very similar in all years. The first discriminant axis usually accounted for about 60-70% of discriminatory power. Depth, sediment particle size, sediment sorting, and salinity, in that order, had the highest coefficients on the first axis. Temperature and salinity were the important variables on the second axis which usually accounted for a further 25-30% of discriminatory power.

3) The species groupings which resulted from the clustering of species were similar from year to year. Consequently, an overall grouping was generated by clustering species on the basis of the proportion of years in which they occurred in the same species group (Figure 3).

4) The relative positions of the species and species groups in the environmental space defined by the first two discriminant axes were highly similar from year to year.

All Years Together

The interannual consistency in the results suggested that it would be reasonable to combine all summer surveys into a single analysis. The scale of the spatial patterns which had emerged, and the observation that depth is the most important environmental variable, suggested that it would also be reasonable to aggregate sets within the survey sampling strata, which are based on depth (Figure 4).

Therefore, a summary cluster analysis was performed using the mean catch-per-tow per stratum over all years for the 31 species used in the previous analysis. The clustering of strata is shown in Figure 5. An interpretation of this clustering has been mapped in Figure 6. There is a high degree of spatial aggregation of strata which cluster together. In general, similar geographical patterns appear in Figures 2 and 6. The differences reflect the different approaches to spatial and temporal aggregation of the data and are typical of the ambiguity in this type of analysis (Gabriel and Murawski 1985).

For practical purposes a more spatially coherent aggregation of strata could be derived by referring back to the matrix of interstratum distances. The decision to allocate a stratum into a particular cluster rather than another which it is closer to spatially may be predicated on small differences in the clustering criterion. Consequently, reallocation of strata from one cluster to another may often be reasonable depending on the objectives of the study. The level of clustering at which groups are defined will also influence the spatial pattern. For example, it may be more appropriate to combine the areas in black and green boxes in Figure 5.

The two-way table in Figure 7 shows the relationship of species abundances to the clusters of strata. For example, the redfish/silver hake species group (VIII) is clearly strongest along the shelf slopes and in the deep basins and adjacent areas in the central shelf. The longhorn sculpin/ yellowtail group (I) is most prominent in the Bay of Fundy and in the Sable Island Banquereau Banks region. The northeast of the shelf is dominated by the cod/plaice group (II) which overlap with the longhorn yellowtail group in the Sable Island; Banquereau portion of its range. The south- western, outer edge of the shelf is an area of mixed species but is characterized by high abundance of haddock, halibut, wolffish, and spiny dogfish.

Discussion

Any kind of 'multispecies' strategy will need to consider the question of management units. Many factors e.g. species distributional patterns, fleet fishing patterns, practicalities of monitoring and surveillance, will have to be considered in defining management units. Here, in the context of a scheme based on assemblage regions and assemblage production units (Tyler et al. 1981), I will consider the relevance of the present study to defining managment units.

Assemblage regions are defined as geographical areas of relatively consistent species composition. Within these regions there may be one or more assemblage production units (APUs); trophically linked groups of species. By analogy to the single species approach, an APU

would be similar to a species, whereas an assemblage region would be similar to stock management area. The basic assumption is that these units would be more functionally discrete than single species. Tyler et al. (1981) suggest that cluster analysis would be a valid way of defining assemblage regions and APUs.

Taking the results of the analysis at face value, the stratum groups could be interpreted as corresponding to assemblage regions and the species groups as corresponding to APUs (Figure 9). The two-way table shows which species groups are most prominent in each stratum group. Clearly if the pattern is predictable it will at least be useful in planning strategies to deal with technological interactions. However, if it is to be useful in dealing with species interactions (predation, competition) then the dynamics of the units described must be more predictable or "better behaved" than the individual elements comprising them. This analysis is, therefore, only a basis for the further study of the dynamics of these units.

At this stage, there is insufficient information on feeding to assess whether the species groups defined here have any functional significance in an ecological sense. The members of a group could simply be responding to the physical environment in the same way, and in fact the distribution of species means in the environmental space does not suggest that the groups are really distinct entities.

This observation takes us towards two questions: 1) whether there are natural groups at all and if indeed the species are not simply distributed along environmental gradients, and 2) how the effects of sample and cluster scale relative to the scale of the ecological processes, have influenced interpretation of the groupings? The first question can be explored by carrying out gradient analyses. If the major environmental gradients can be defined, one could analyse the distribution of species means, and end points on these gradients to determine if the species were nonrandomly distributed along the gradient (see Gardiner and Haedrich 1978). The second question is more difficult. To begin with, a trawl could combine in one sample many species which may never encounter each other under natural conditions. There is also the situation of the natural range of spatial scales which may vary one or two orders of magnitude. For example, the contrasting distributional scales of the spiny dogfish and the winter flounder on the east coast of North America.

In conclusion it appears that there is a persistent spatial pattern of groundfish distribution on the Scotian Shelf. This pattern appears to be strongly influenced by the physical environment, perhaps much more so than by species interactions. The existence of regions which are relatively homogeneous with regard to species composition can be useful in dealing with bycatch problems (Brodie 1985; Gabriel and Murawski 1985; Sinclair 1985). However, we are at a very early stage as regards defining ecologically based management units in the area, if indeed these exist.

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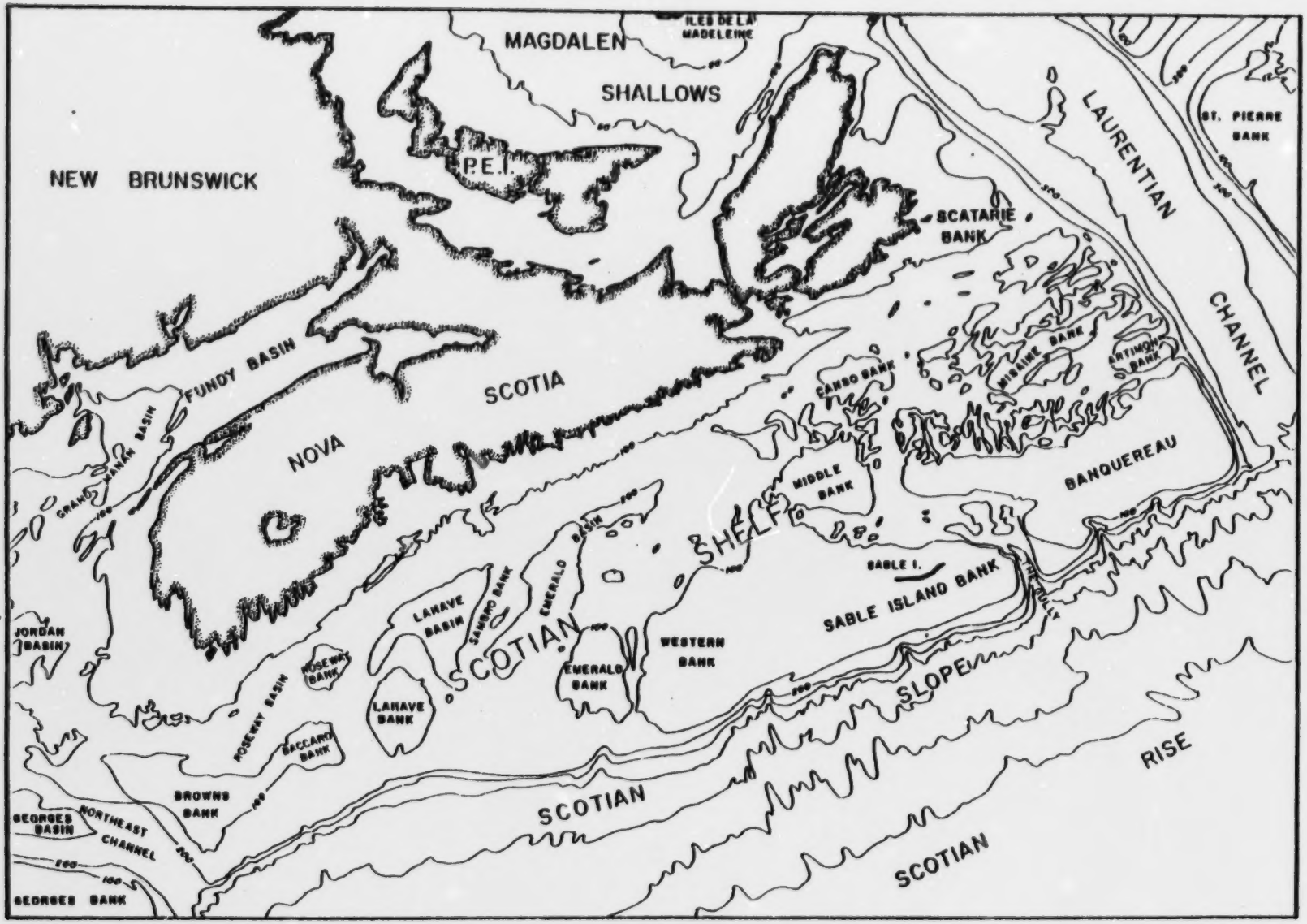

Figure 1. The Scotian Shelf and Bay of Fundy

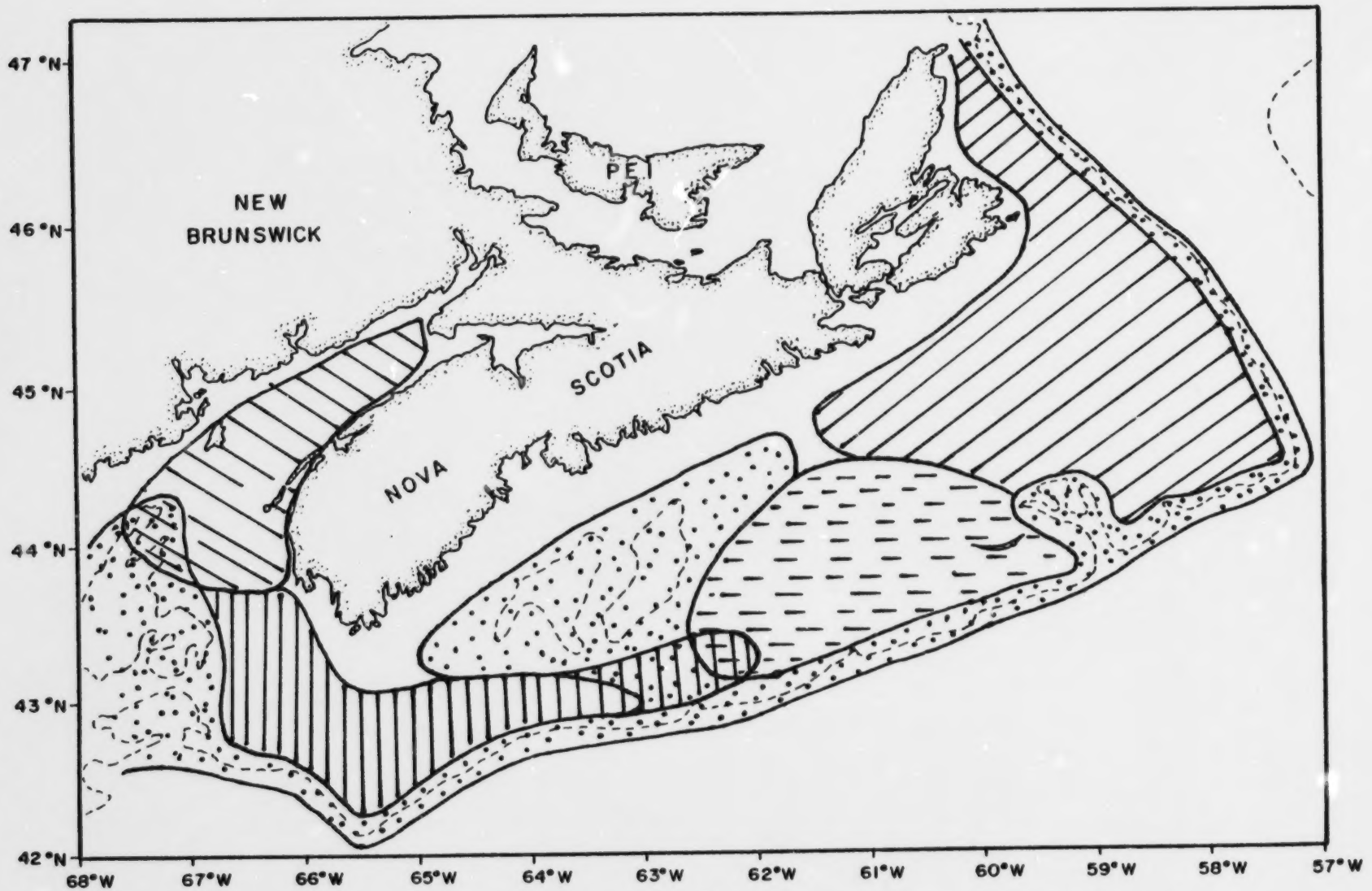

Figure 2. Areas within which sets tended to cluster together year after year (drawn by eye)

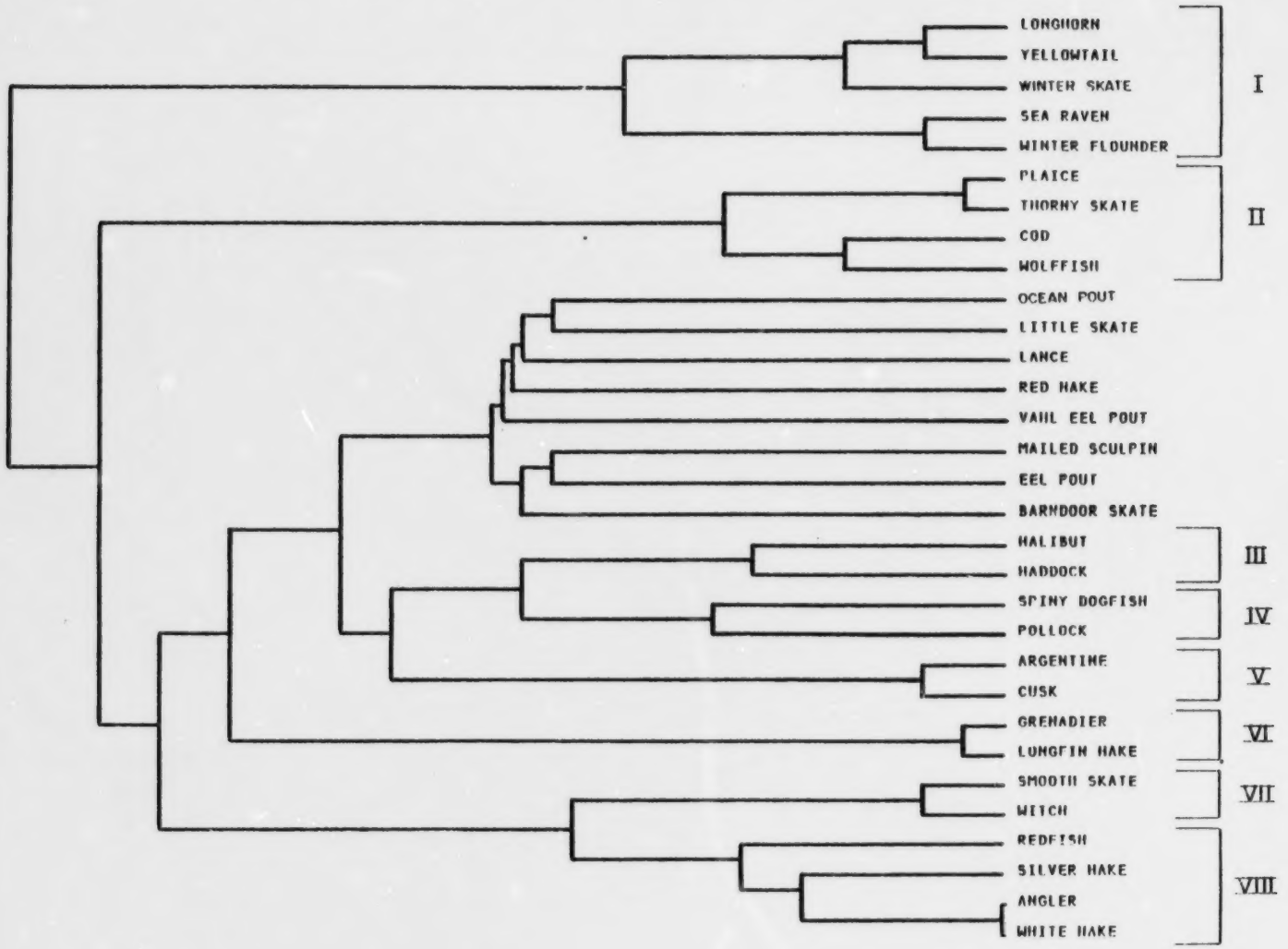

Figure 3. Composite species dendrogram (summer 1970-1981) based on the proportion of years in which species occurred in the same group. Eight groups or pairs of species were recognised.

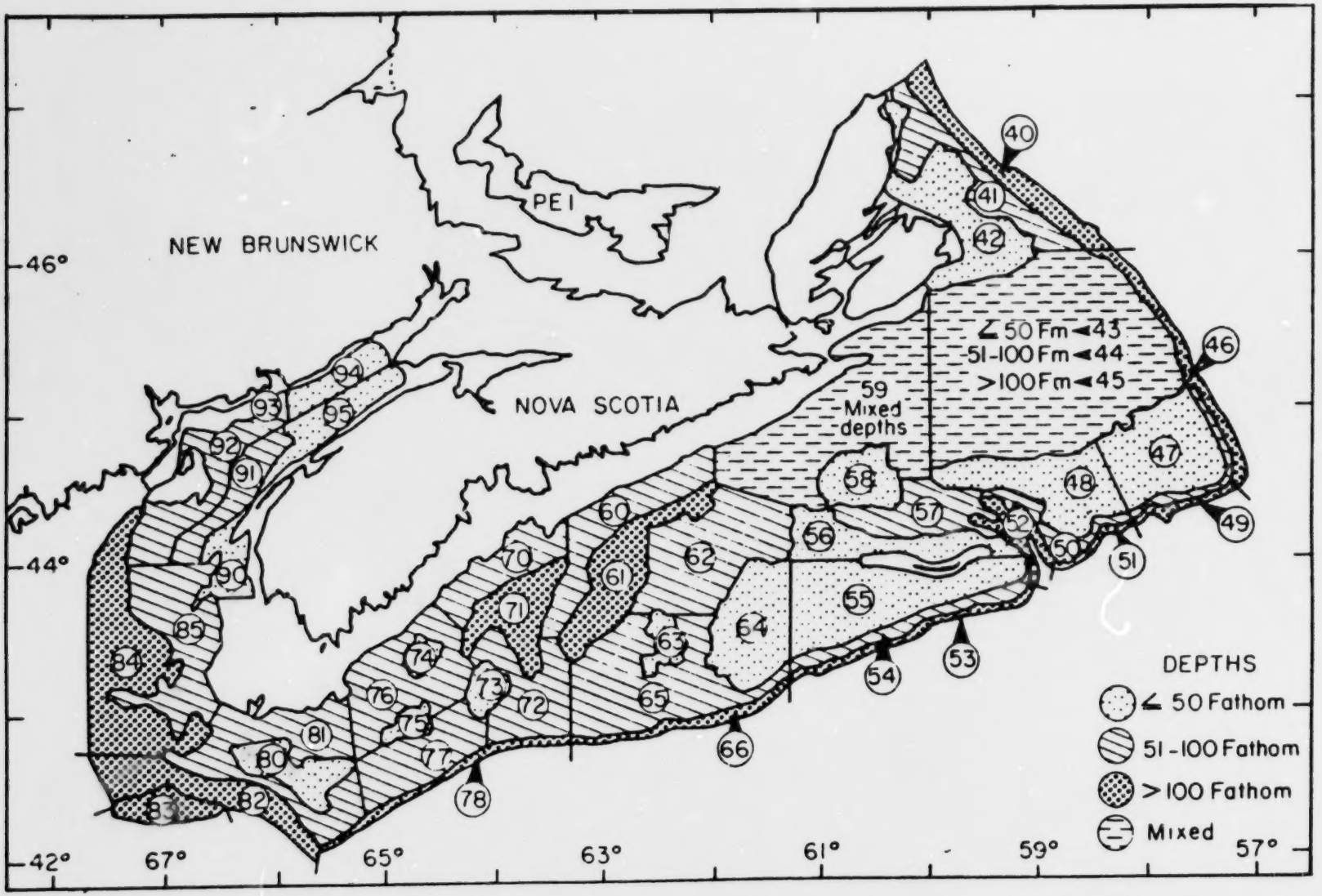

Figure 4. Groundfish trawl survey sampling strata on the Scotian Shelf and in the Bay of Fundy

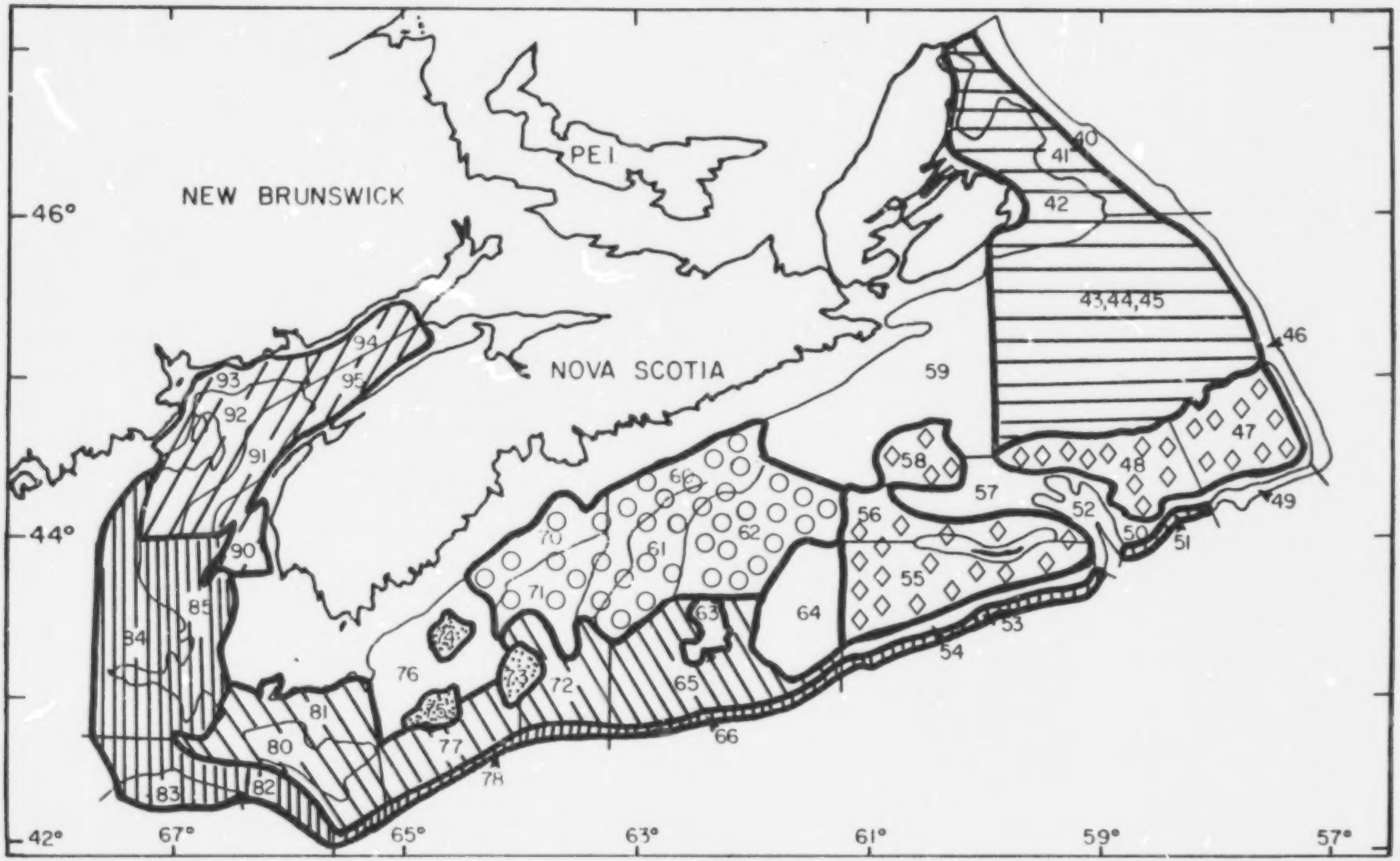

Figure 5. Groups of strata which clustered together on the basis of species composition averaged over all years (1970-1981). Unshaded strata belong to a mixed group which could be allocated to other groups.

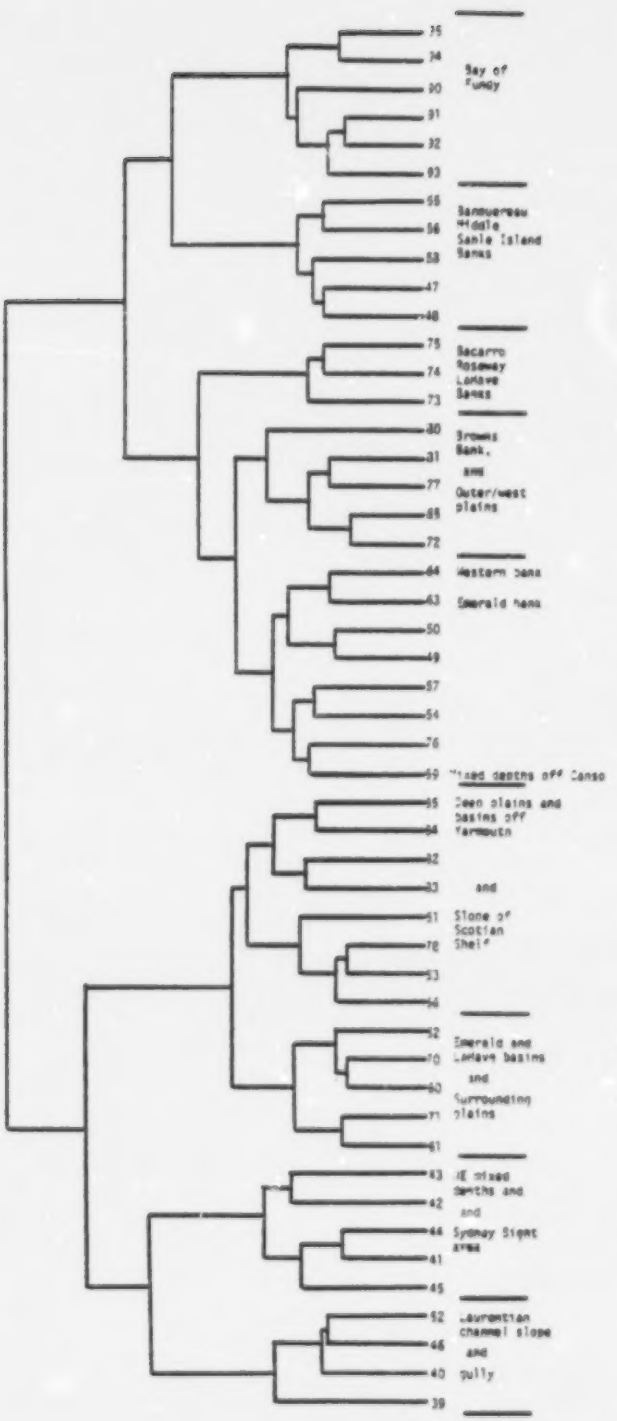

Figure 6. Dendrogram showing clustering of survey strata on the basis of abundance of species averaged over 1970-1981. The stratum numbers correspond to those in Figure 4

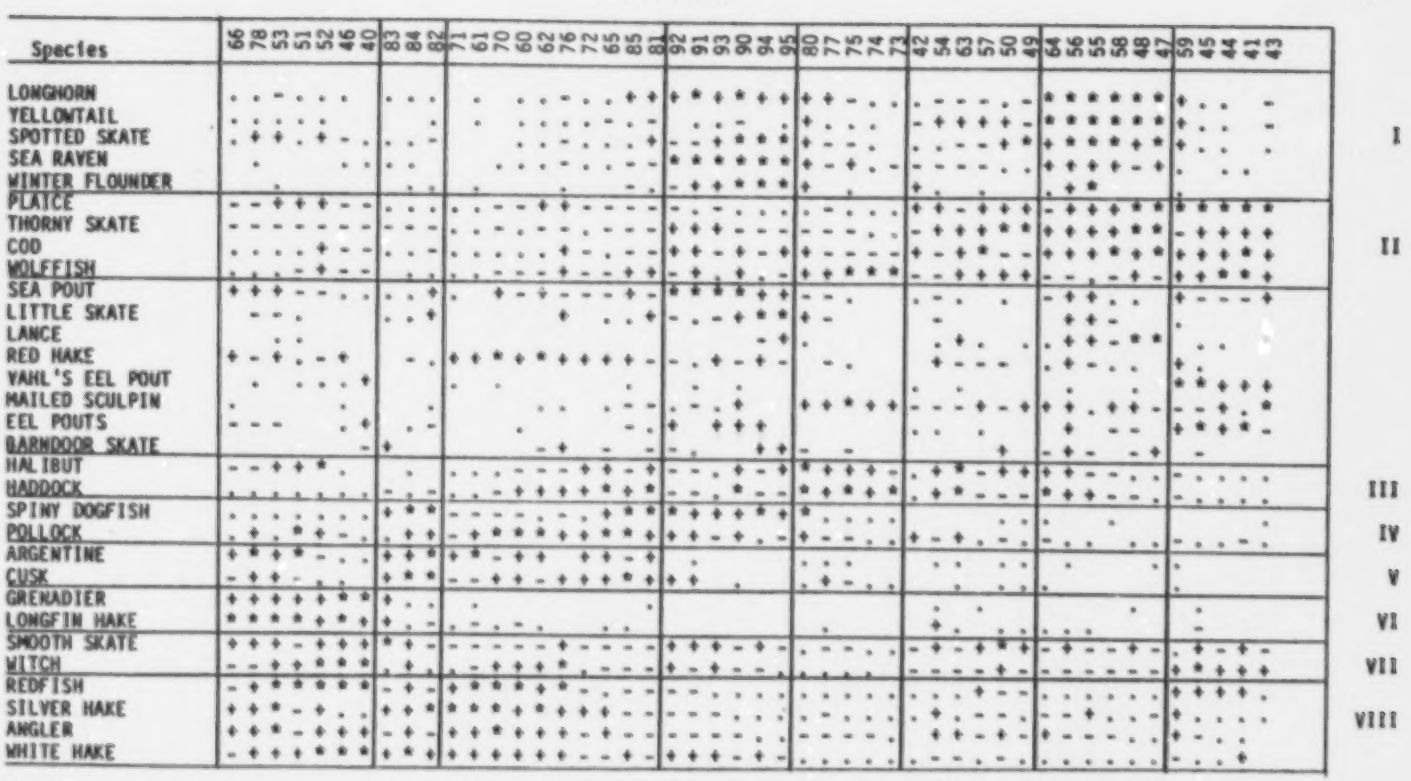

Figure 7. Two-way table of species abundances (see symbol table) in stratum groups. The strata area arranged as in Figure 6, the species as in Figure 3. The vertical lines separate the clusters shown in Figure 6, the horizontal lines separate the species groups (GRP) in Figure 3.

Environment on the East Coast and in Particular the Scotian Shelf

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A brief overview of the Scotian Shelf oceanography is presented. Large-scale patterns in sea surface temperature (SST) variability in the Northwest Atlantic are examined using empirical orthogonal function (EOF) analysis. It is suggested that SST's are influenced both directly and indirectly by meteorological forcing. On the one hand, winter winds constitute a major direct influence, while on the other, river runoff with subsequent altered water column stability and the broad ocean gyres and meanders and rings of the Gulf Stream provide indirect pathways.

Introduction

At a previous workshop of the Marine Environment and Ecosystem Subcommittee of CAFSAC on the "Fisheries Ecology of the Scotian Shelf" held in October, 1981, a session was devoted to a discussion of physical oceanographic features and processes on the Scotian Shelf and the possible relationships to biological events. It focussed on seasonal temperature, salinity, and circulation patterns, the development and existence of fronts, and meteorological and offshore forcing mechanisms. During the past three years, research has continued to be focussed on the southwestern part of the Shelf, providing a better description and understanding of the mean and seasonal circulation (Smith, 1983), of the current response at the shelf break to transient wind forcing (Petrie, 1983), and of the role played by tides in generating residual currents (Greenberg, 1983). In this paper presentation of the physical oceanography of the Scotian Shelf is limited to a summation of the important forcing mechanisms. Attention is focussed on the large scale circulation pattern, the sea surface temperature variability in the Northwest Atlantic and the effects that the large-scale meterology may play on oceanography, both directly and indirectly.

Scotian Shelf Environment

Low salinity water flows onto the Scotian Shelf from the Gulf of St. Lawrence through Cabot Strait. It is more than a tracer and appears to be a major force driving the flow southwestward along the coast (Petrie, Personal Comm.) with maximum speeds in the 0.05-0.15 m s^{-1} range mainly confined to the inner third of the Shelf. Local freshwater discharge is low and is relatively unimportant to the Shelf dynamics. The pulse of low salinity water that is carried southwestward in the Scotian Current, reaches the Cape Sable area in the November-January period and is coincident with maximum transports.

Meteorological forcing is important on at least two time scales. Seasonally the wind speeds are stronger in winter than in summer and there is also a significant change in prevailing direction. Both current meters and drifters confirm that seasonal changes occur in the circulation over the Scotian Shelf (at least in the surface layer). Wind forcing on a 2-10 day time scale is also very important on the Shelf; some of the more dramatic manifestions being coastal and shelf-break upwelling. Petrie (1983), using current meter data from an array of moorings situated on the southwestern Scotian Shelf at its outer edge, and wind data from Sable Island, found that transient winds blowing parallel to the bathymetry and at moderate speeds, could induce upwelling at the shelf break from depths of 400 m and with peak vertical velocities of about 2 mm s^{-1}. The upwelling appears to be confined to within 10 km of the shelf-slope break. Both topography and stratification play important roles in determining the nature of the wind-induced responses.

Two kinds of fronts influence the Scotian Shelf oceanography. The first delineates the boundary between Shelf Water and Slope Water and is, to a first approximation, parallel to the Shelf break. Its position at the surface is on average more than 100 km offshore from the Shelf

break but can vary by as much as ± 100 km in the onshore-offshore direction. The second kind, tidal fronts, occurs seasonally and results from spatially varying tidal currents. The front marks the boundary zone between areas where the tidal currents are strong enough to mix the water to near-homogeneity vertically and the surrounding water that remains stratified. Although the physical mechanisms involved are different for the two kinds of fronts, both are areas where primary production is enhanced.

Offshore forcing, imposed by large-scale oceanic conditions, sets up pressure gradients along the Nova Scotia coast and appears capable of producing currents of comparable magnitude and direction to the surface-layer circulation induced by freshwater (Csanady,1979). Additionally, offshore forcing, arising from Gulf Stream warm-core eddies interacting with the shelf water, occurs on time scales of weeks, and gives rise to major onshore and offshore excursions. Although the full geographic extent of these excursions is not known, they are clearly important on the southwestern Scotian Shelf and at least as far to the northeast as Emerald Bank. An indication of the time and space scale that offshore forcing may operate on is evident in Figure 1. A large fraction of the surface layer of the southwestern Scotian Shelf may leave the shelf and subsequently be incorporated into Slope Water. The time scale of such "events" may vary from days to weeks - comparable to that of the larval phase of many marine species.

Smith (1978) estimated that the volume of Scotian Shelf water exchanged with off-shelf water over a 2-month period in 1976 during one eddy interaction event was of the order of 2×10^3 km^3 and that on the basis of 6 events per year would represent an offshore transport of 1.2×10^4 km^3/yr. In terms of pure shelf water the amount was estimated at 0.5×10^4 km^3/yr. This would suggest that a volume equal to about half the mean annual geostrophic transport southwestward through the Halifax Section (Drinkwater, et al. 1979) may be drawn off the Shelf through eddy-shelf interactions.

From time to time Slope Water penetrates, at depth, across the saddle between Emerald and LaHave Banks resulting in replacement or partial replacement of the deep, warm, salty water in Emerald and LaHave Basins. Offshore waters have a more limited and less direct access to the basins in the northeastern Shelf area, with the result that the T-S properties of the basins are similar to those at intermediate depths in the Laurentian Channel.

Both the circulation and distribution of properties are influenced significantly by the topography: the effect of the coast may be to produce either upwelling or downwelling; upwelling may be induced along the Shelf break; strong tidal flows over banks may give rise, through tidal rectification, to mean flow around a bank with a "jet" located near the edge of the bank. For the Scotian Shelf, it appears that an observable tidal rectification effect is likely confined to the Browns Bank area where tidal currents are moderately strong.

With respect to temperature and salinity properties, Scotian Shelf waters are derived principally from the outflowing waters from the Gulf of St. Lawrence and Slope Water augmented, at times, with a small quantity of Labrador Current Water flowing directly across the Laurentian Channel from the Grand Banks. Although the T-S properties observed on the Halifax Section are customarily considered as representative of conditions on the Scotian Shelf, they in fact should not be considered as representative of more than the middle portion of the Shelf, since the "source" waters, the important driving forces, and the topography all vary markedly over the Shelf.

Spatial and temporal variability must be kept clearly in mind when investigating physical-biological interactions. In many respects surface currents are as variable as the wind. Although temperature shows a clear seasonal cycle down to depths of convective cooling, some areas display a week-to-week variability comparable in magnitude to the year-to-year fluctuations in the monthly means. Caution must be exercised as well in assuming that conditions in one area of the Shelf apply even qualitatively to another area. Although long term trends (averages over several years) in sea surface temperatures typically do show close similarities over many hundreds of kilometers, averages over periods of a year or less may have limited areal coherence. In particular one must exercise prudence when using inshore monitoring sites (such as Halifax Harbour temperatures) to represent Scotian Shelf conditions.

Figure 2 depicts an attempt to summarize visually a perception of the spatial variation in importance of the various oceanographic forcing mechanisms operative on the Scotian Shelf. Care, however, must be exercised not to overinterpret such a schematic. For example, although warm-core eddies have much more important influence on the southwestern Scotian Shelf than in the northeastern area, it should not be interpreted that the warm-core eddies are any more, or any less, important than the tides.

Northwest Atlantic Environment

The general surface layer circulation in the northwest Atlantic is shown diagrammatically in Figure 3. The Scotian Shelf is essentially

embedded in southward moving coastal water. As the water moves southward it generally becomes progressively warmer and saltier by the action of solar heating and mixing with offshore waters. Important exceptions are the freshening influences of the large discharge of low-salinity water from Hudson Strait and the Gulf of St. Lawrence owing to the large rivers entering these systems. Peak discharge from the Hudson Bay system is readily observable as a salinity minimum at Station 27, 10 km off St. John's, Newfoundland while the pulse from the St. Lawrence river system is traceable from salinity through the Gulf of St. Lawrence and along the Scotian Shelf at least to the southern tip of Nova Scotia.

The seasonal patterns of sea surface temperature (SST) and its progressive changes along the continental shelf from Cape Hatteras to southern Labrador may be seen by examining Figures 4 and 5. Interestingly, from about the Laurentian Channel southward the summer-winter difference is remarkably constant, except in the Gulf of Maine area where increased tidal mixing diminishes the amplitude. From the Laurentian Channel northward the amplitude of the seasonal cycle diminishes owing to ice formation and decreasing solar heating.

Examination of the standard deviation of the monthly ocean temperatures reveals values in the neighbourhood of 1°C are common and that a peak of about 2°C occurs at the time of maximum heating. Monthly anomalies of above or below "normal" temperatures often extend over a relatively large area and may persist for many months (Fig. 6). Additionally, there is some indication of a tendency for temperature anomalies in the area from the Gulf of Maine northward to be out of phase with those of the areas to the south.

Grouping SST's into 24 areas (Fig. 7), computing annual SST anomalies, and further smoothing by contouring only values greater than $\pm$ 0.15°C sharpens the impression that the southern and northern areas, except for the Labrador Sea-Cape Farewell region, tend to respond with opposite phase (Fig. 8). Changes in SST on geographic scales of a few thousand kilometers, which may occur nearly simultaneously but may persist for many months, imply that the meteorology may be the large scale mechanism involved. This will be further addressed in the following section.

Patterns in Sea Surface Temperature Variability

The relatively large SST data base ("Marine Deck") allows studies of spatial and temporal variability and a search for possible relationships with other parameters such as freshwater discharge, air temperature and pressure, and winds. The "Marine Deck", which is a set of weather and sea observations principally collected from cooling water intakes of merchant ships and archived at the National Climatic Center, Asheville, North Carolina, was obtained for the northwest Atlantic (latitudes 30°N to 60°N and west of 40°W) from the time of earliest observation up to 1981. This data base has been previously used by us to calculate monthly SST anomalies by squares and by fishing bank areas in an overview of conditions for the 1970 - 79 decade (Trites,1982). More recently (Loucks and Trites 1984) we have undertaken further analyses by first grouping the data for selected areas. The areas, shown in Figure 7 were chosen to include fishing banks and portions of large oceanographic features such as the Gulf Stream, Labrador Current, etc. In addition to undertaking space-time plots of annual anomalies, spectral analysis, computation of variances, correlations among monthly anomalies and empirical orthogonal functions (EOF's) by season, attempts are made to interpret the results in terms of wind effects, offshore forcing and river discharge.

Over the 40-year period, 1940 - 1980, the annual anomalies of SST (Fig 9) show that the late 1940's and early 1950's was a period of above normal temperatures which persisted for several years and occurred throughout the entire area. In the mid 1960's most of the area experienced below normal temperatures while in the last decade there has been a tendency for the northern and southern areas to behave in opposite phase (see also Figure 8). Figure 10 shows the deseasonalized variance in $(°C)^2$ of the monthly SST's over the period 1946 - 80. Four zones can be identified. (1) The Slope Water areas show the highest variances. The power spectra here have two peaks - a minor peak at two months and a major one at 12 months and greater. The high variability in the area is not surprising as warm core Gulf Stream rings with life spans typically of a few months form or move through the area, entraining cooler shelf water. The variance in this region may arise in large measure from a combination of meteorological forcing and the effects of Gulf Stream meanders and rings. (2) The Labrador coast area shows approximately one-half the total variances of the Slope Water areas. The power spectrum values for the Labrador coast area are small at low frequencies but shows a strong peak at two months. (3) The coastal regions of the Scotian Shelf and the Grand Banks show intermediate variances - attributable, we conjecture, to a combination of meteorological forcing and river runoff effects. (4) The Gulf Stream and Sargasso Sea areas show the least variance, the least anomalous and most regular SST signals.

An EOF analysis has been made of SST with resolutions of 3 months and 100 to 1000 km.

Figure 11 shows the first mode EOF pattern for winter. The first mode shows some similarities in all seasons and explains 27 to 40% of the variance in the whole SST field. It represents uniform increase (or decrease) in SST coherent over a large geographic scale. The corresponding first-mode time series, spanning the period 1950 to 1980, has been regressed with seasonal geostrophic winds calculated by Keith Thompson (Dalhousie University, personal communication) for five positions along the seaboard. In winter as much as 45% of the first mode variance can be accounted for by the winds, (offshore wind anomalies are associated with negative SST anomalies) suggesting that winter winds may be one of the more important driving forces and that large-scale wind patterns may account, in large part, for the first-mode SST patterns.

The pattern of eigenvector components for the second mode EOF is similar in all seasons and explains 19% to 22% of the variance in the SST field. This mode shows peaks over the Grand Banks and Southern New England/Mid-Atlantic Bight areas ~180 degrees out of phase. A tentative interpretation based on preliminary results is that this mode is related to (and generated by) a similar second mode (modulation) in the geostrophic wind field.

Evidence for indirect pathways between meteorology and SST's (via river runoff) is provided in Figure 13 which indicates a positive correlation between the north component of geostrophic winds on the Scotian Shelf in winter, and the anomaly of annual discharge of the St. Lawrence River System (RIVSUM) in the same year. The spatial scale of weather patterns is such that one might reasonably expect anomalous winds on the Scotian Shelf to be associated with anomalous precipitation patterns over the St. Lawrence drainage basin and in turn to affect subsequent freshwater discharge in the rivers. The notion that freshwater discharge and offshore forcing may have common parentage (in winter meteorology) is supported in Figure 14. Winter temperatures from the Western Slope Water area show close correspondence with RIVSUM from the previous spring. This surprising relationship suggests an answer to the puzzling observation (Sutcliffe et al. 1976) that although SST anomalies are coherent and advectively linked down the Atlantic seaboard, yet positive temperature anomalies are associated with salinity mimima on the Scotian Shelf and salinity maxima off New England. If perturbations in the discharge from Gulf of St. Lawrence rivers and in temperatures of the Slope Water have common parentage in large-scale atmospheric variability and if, where they meet in the vicinity of the Gulf of Maine area, the temperature anomalies are of the same sign, albeit arising via different circumstances, then the large-scale coherence of SST anomalies follows. Specifically, the evidence suggests that the winter meteorological perturbation which produces high (low) spring freshet and high (low) summer and autumn SST's along the Scotian Shelf also produces enhanced (diminished) influence of the Slope Water and therefore high (low) salinities together with high (low) SST's in New England waters during the second winter.

A schematic illustration that summarizes our impressions that meteorological forcing is the primary driving force for SST's and that this meteorological influence acts directly as well as indirectly is shown in Figure 15. The direct influence is believed to be greatest in winter, the season of strongest winds (centre path Fig. 15). The indirect pathways involve lags. The left-hand path in Figure 15 represents the sequence whereby a variation in winter winds accompanies a variation in snow accumulation over the drainage basins of the St. Lawrence and Hudson Bay rivers, thus setting the stage for a variation from normal in the spring freshet and in the summer and autumn SST's of coastal waters as reported by Sutcliffe et al. (1976). The right-hand path in Figure 15 is the more speculative one. It is suggested that interannual variations in winter winds perturb, through the wind stress curl, the circulation in the great ocean gyres of the North Atlantic and that these perturbations subsequently affect the SST's in the Slope Water region.

At present we are seeking to express the variations in the SST field as a set of superimposed, propagating 'waves' of different frequencies; this is in contrast to the standing wave analysis of EOF's reported above. Using cross-spectra and 'complex' EOF's, we hope to trace the (direct path) meteorological 'wave', and the (indirect) river runoff and offshore forced 'waves' through the SST field.

Acknowledgements

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Discussion Period

Sandeman: How deep is the influence of the warm core eddy when it impacts on the Shelf?

Trites: Probably the top 50 metres or so.

Smith: Yes, however, when the surface water is drawn off the shelf it is replaced by deeper water which comes up over the shelf edge providing nutrient flux to the shelf water.

Akenhead: Is it possible that some of the differences that Robin Mahon saw in species assemblages are paralleled by the vulnerability of certain banks to warm core rings (Banquereau versus southwest Grand Bank)?

Smith: Sixty five degrees west seems to be a preferred location for the formation of these rings and present knowledge is that they seem to be the result of hydrodynamic instability and/or sea mounts off Georges Bank. Consequently, they have their greatest effect on the western end of the Scotian Shelf, and gradients in the hydrodynamic properties along the Scotian Shelf show the effect of mixing in slope water as the flow moves westward along the shelf. Our recent experience shows that important cross-shelf exchange processes are associated both with warm-core rings and with wind-driven shelf break upwelling in winter.

Sandeman: Why do the rings go southwestward?

Smith: There is a natural tendency for those types of vortices to propagate westward. Planetary dynamics require that on a flat bottom the motion of the rings would have a net westward component. However, bathymetry imposes a similar constraint resulting in mean motion along isobaths in a westward direction. In practice, the rings formed near 65°W are observed to move westward between the Gulf Stream and shelf break until they are absorbed by the stream.

Trites: Could you comment on the eddies off the southwest Grand Banks region, we have much less information about these eddies. They are being formed further east and do not appear to have as long a life span as those formed in the 65°W area.

Smith: Just that they are also probably hydrodynamic instabilities of the Gulf Stream. There has been some field work and eddy resolving models show eddy formation shortly after the Stream leaves Cape Hatteras and then again at the Tail of the Bank or some bathymetric features there and turns northward inducing other transient behaviour. I do not know that they are more tightly constrained by the bathymetry and the current, which seems to control the lifetime of the rings which are formed to the west also. The natural diffusion within the slope water would take two or three years to get rid of large dynamical features, but more often they are absorbed by another passing meander.

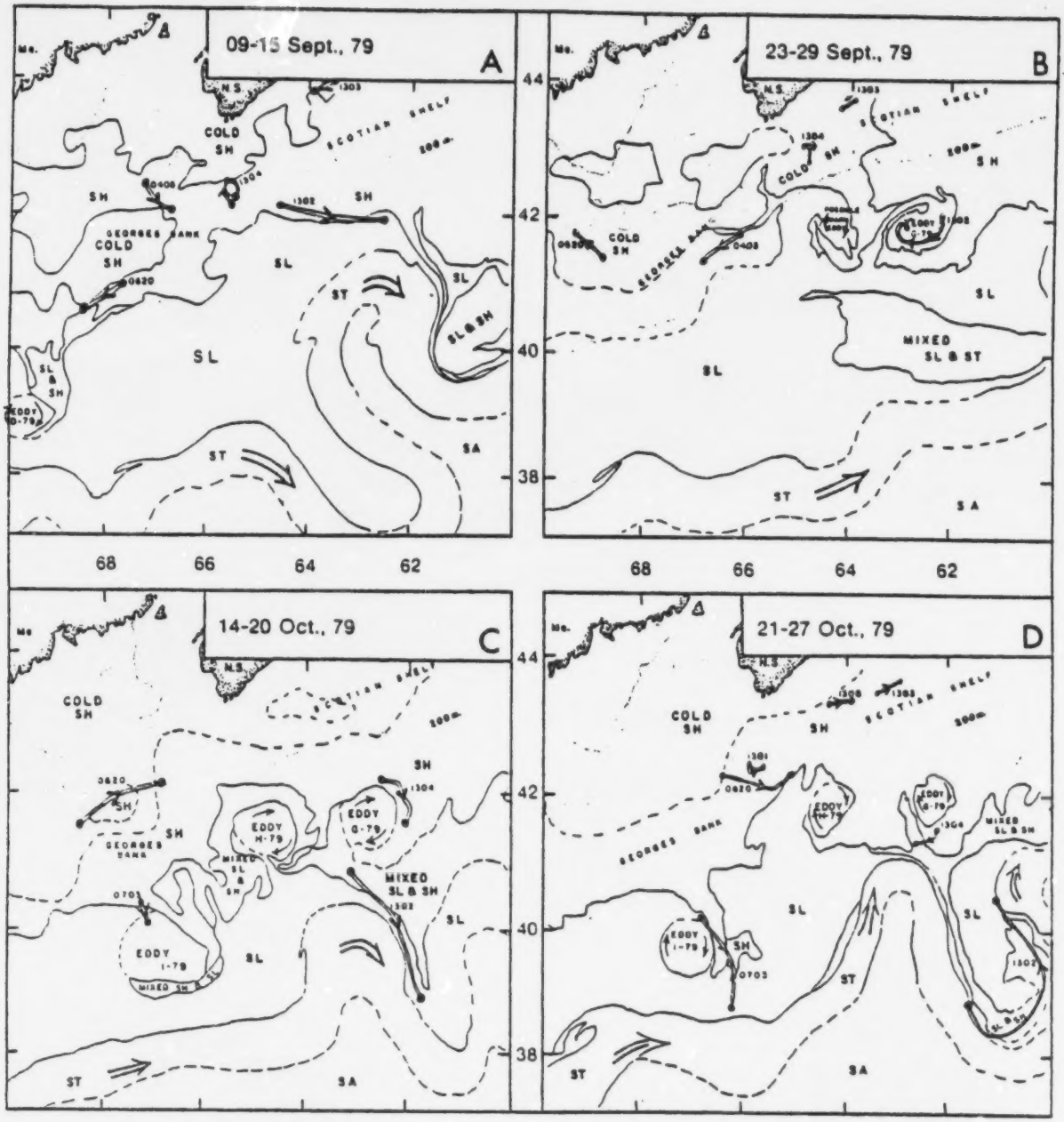

Figure 1 Maps showing surface thermal features for weekly periods in 1979, extracted from the U.S. Naval Oceanographic Experimental Ocean Frontal Analysis Charts. A, 9-15 September. B, 23-29 September. C, 14-20 October. D, 21-27 October. (Key for water types: SA = Sargasso Sea; ST = Gulf Stream; SL = Slope Water; SH = shelf water; COLDSH = cold shelf water. Approximate trajectories of buoys for each 7-day period are also shown).

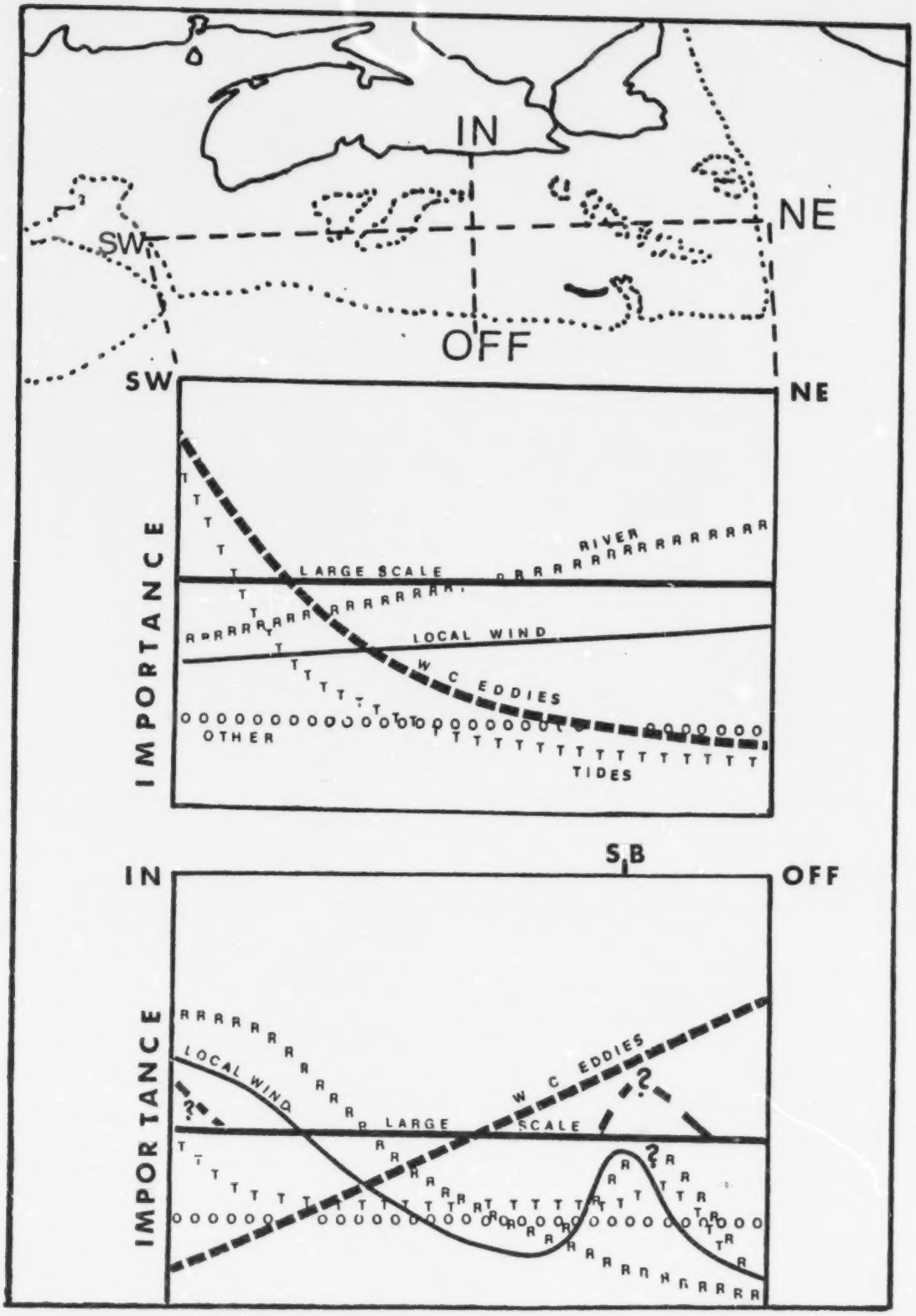

Figure 2 Schematic illustration of the spatial variations in importance of the various forcing mechanisms operative on the Scotian Shelf.

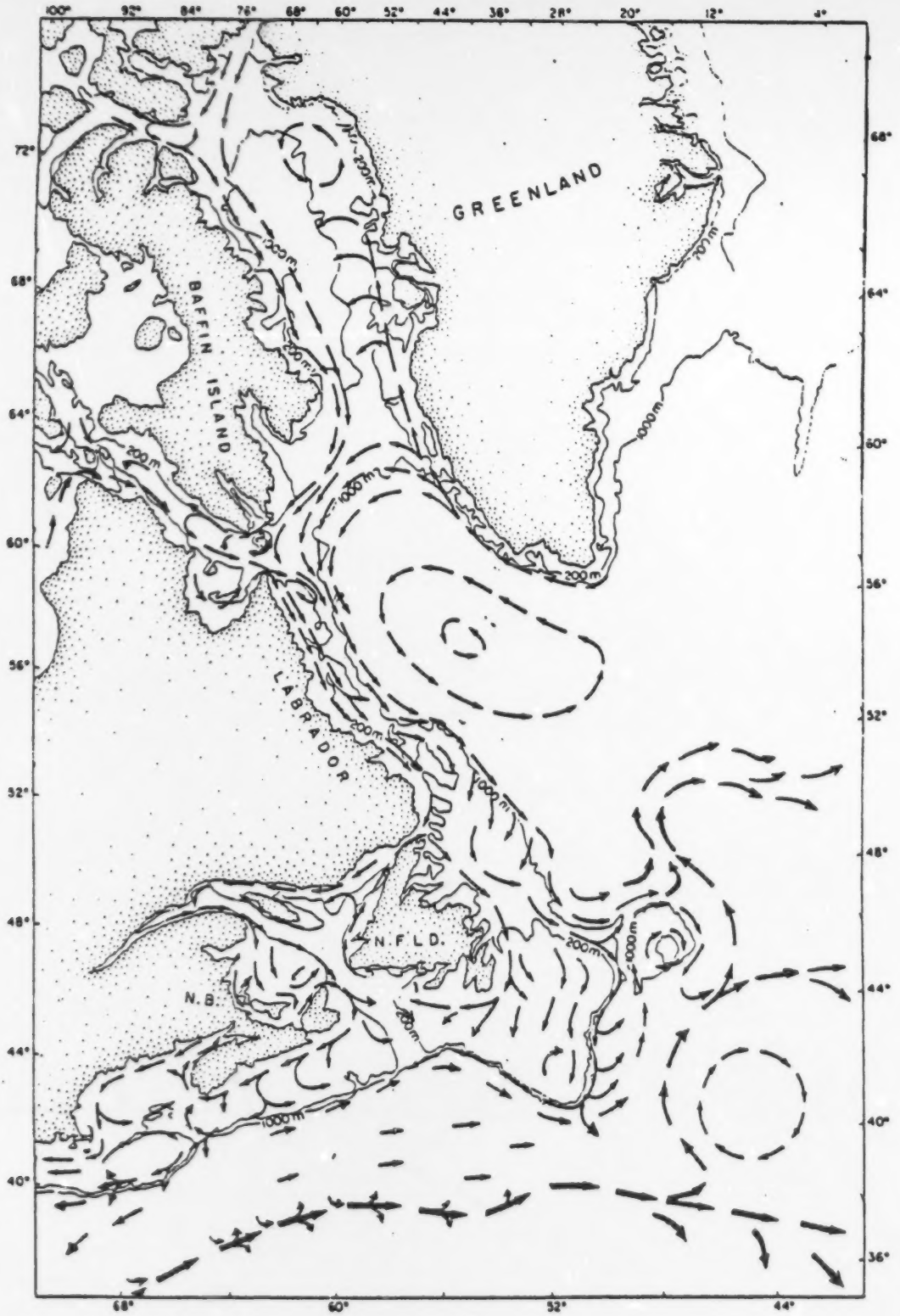

Figure 3 Schematic illustration of surface circulation features in the northwest Atlantic Ocean.

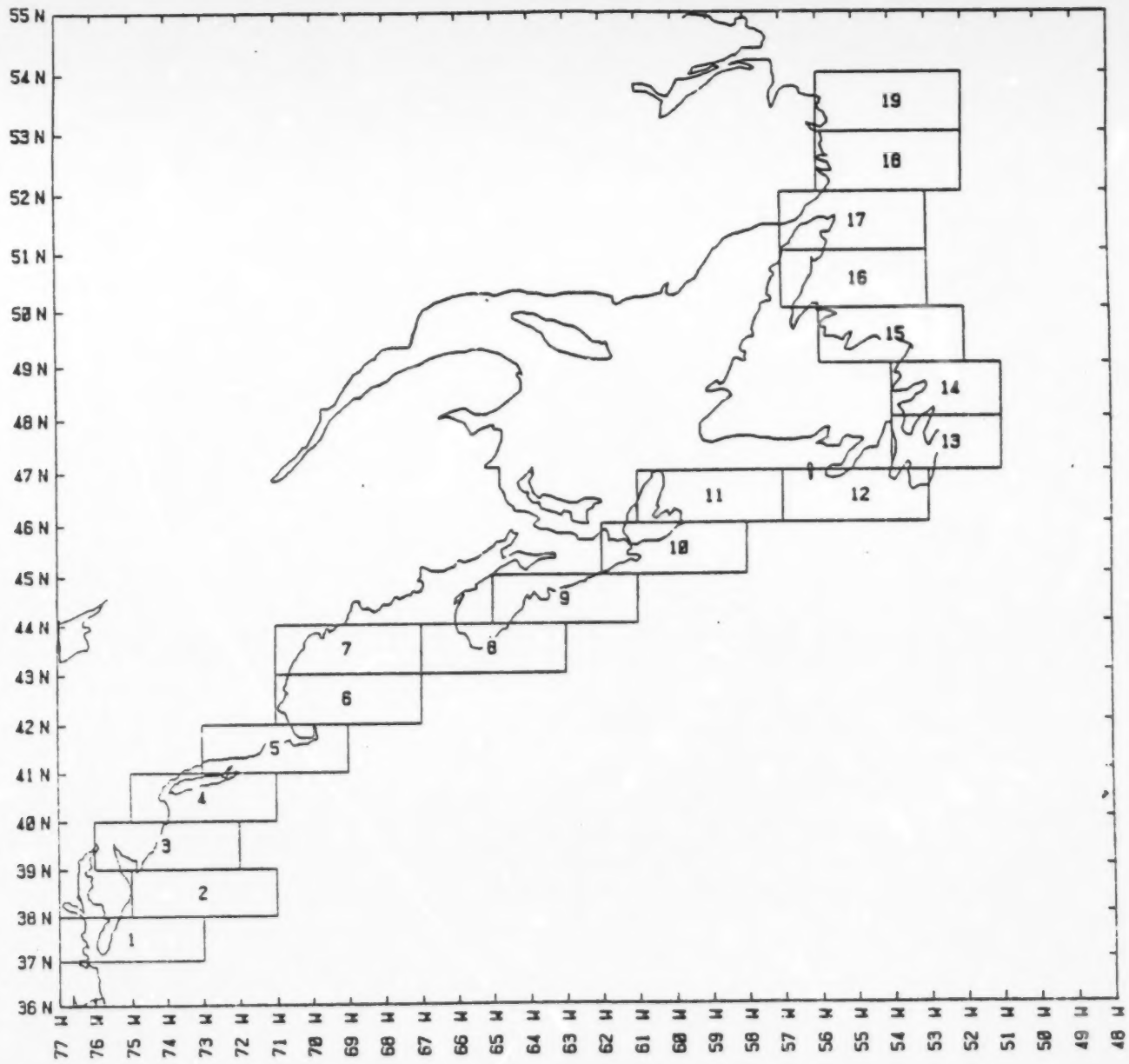

Figure 4 Georgraphic location of the 19 coastal subareas where sea surface temperatures have been grouped between Cape Hatteras and southern Labrador.

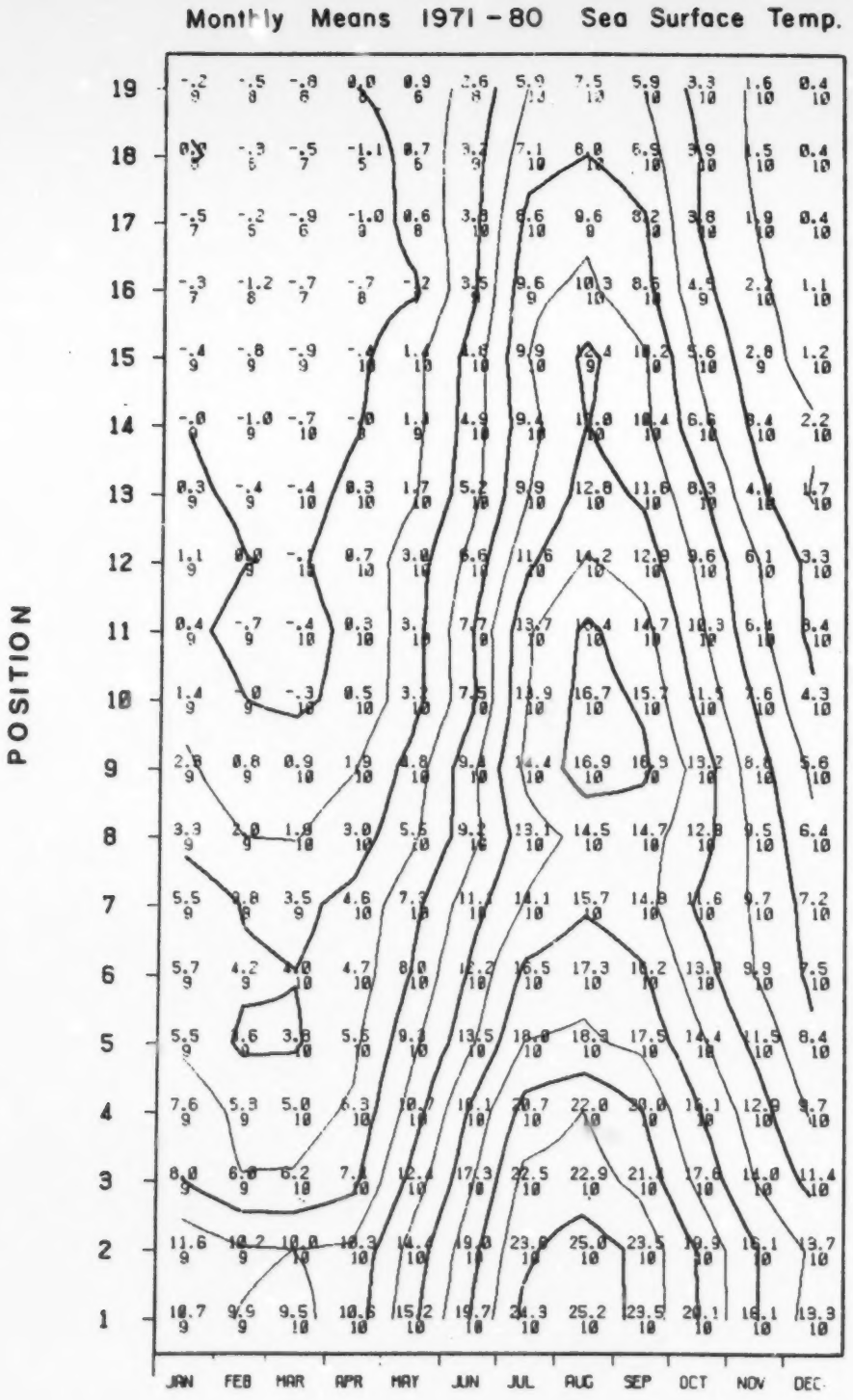

Figure 5 Plot of the mean monthly sea surface temperatures for the period March 1971-December 1980 for the 19 subareas along the coast. The upper number is the temperature in ^{o}C, and the lower is the number of years for which data were available.

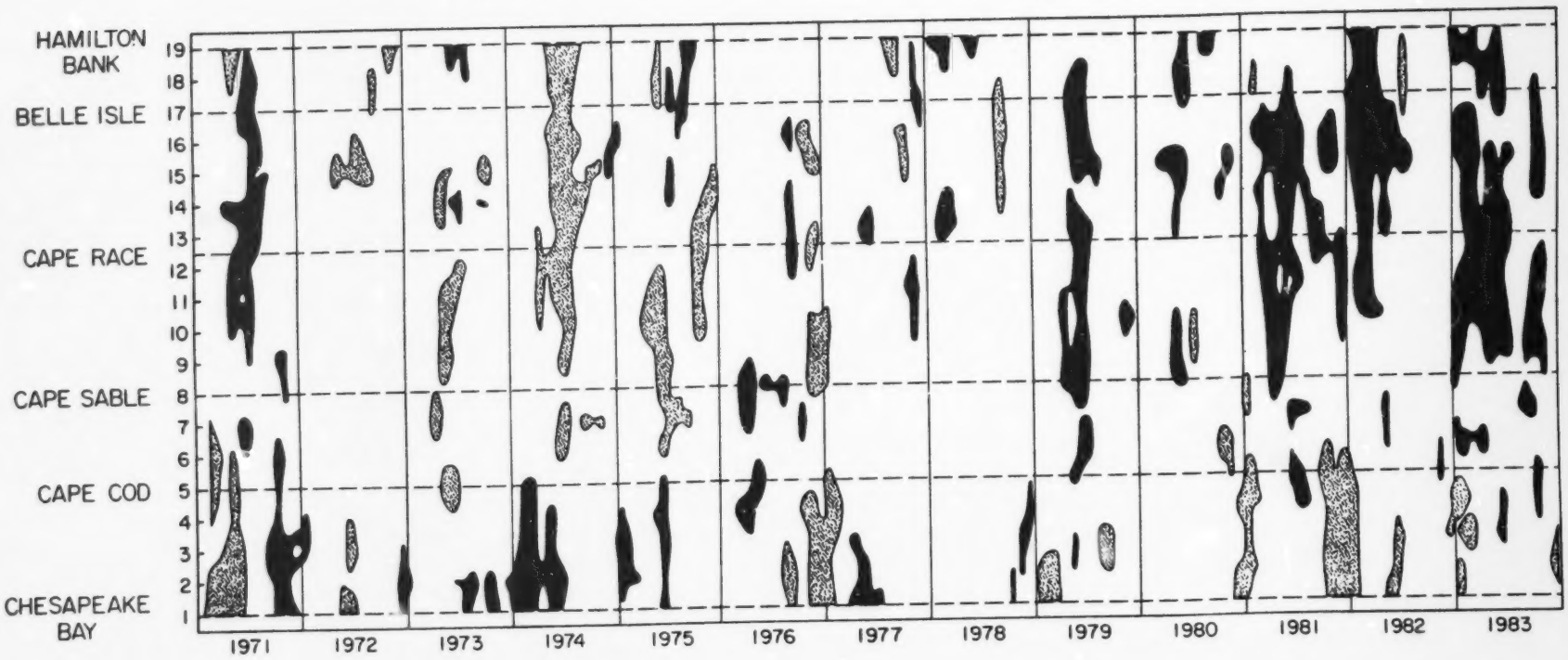

Figure 6 Monthly sea surface temperature anomaly (using mean for March 1971-December 1980 as normal) for each subarea. Only anomalies in excess of 1°C and extending in space through at least two neighbouring areas or in time for at least two consecutive months have been contoured.

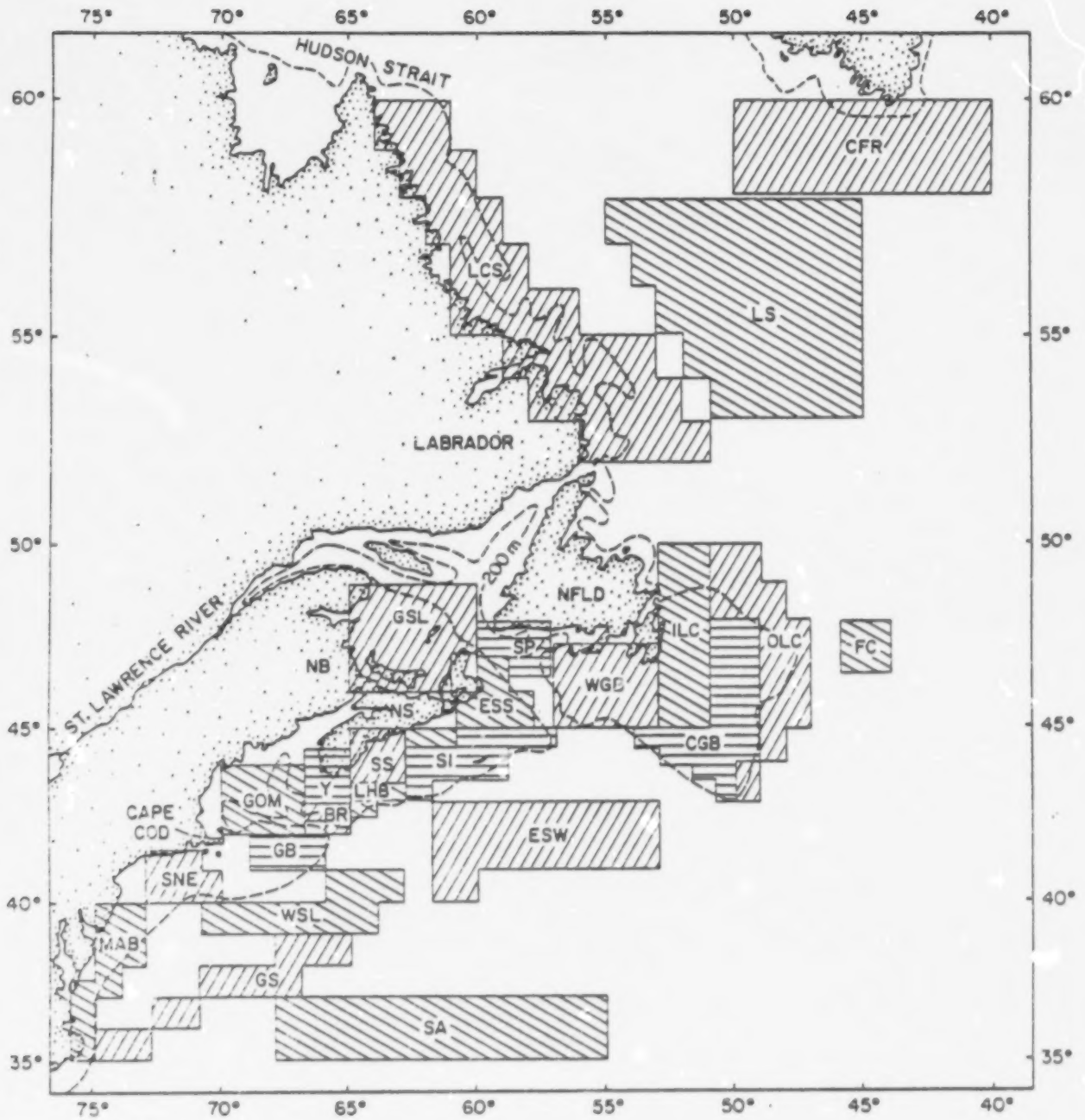

Figure 7 Chart of Northwest Atlantic showing 24 areas over which SST observations have been averaged monthly.

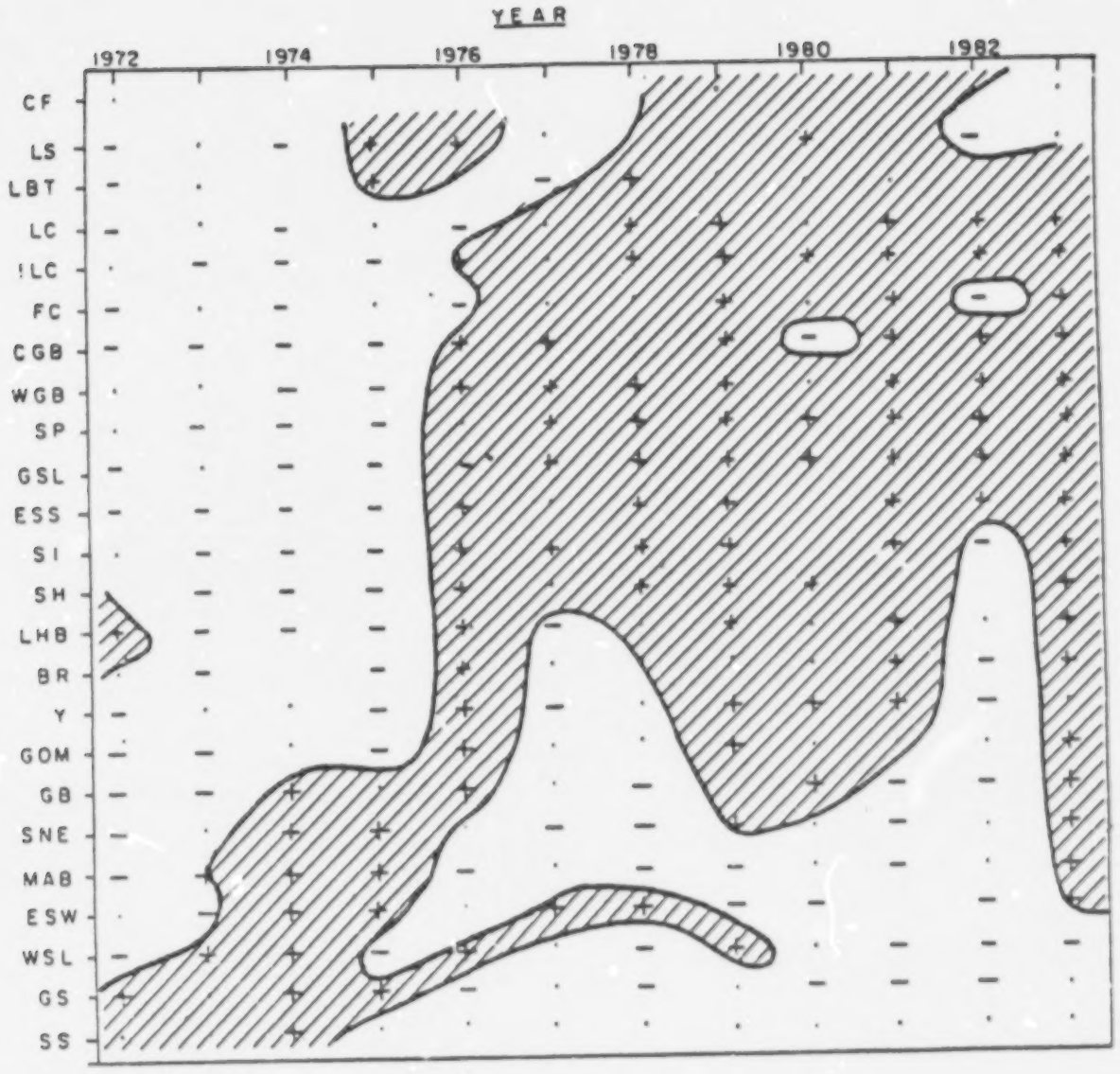

Figure 8 Distribution of annual sea surface temperature anomalies in 1972-83 by subareas (see Fig. 5) relative to the means for the 1972-80 base period. (A "+" or a "-" symbol represents anomalies which exceed 0.15°C, and a "." represents anomalies with a magnitude less than 0.15°C). Only anomalies exceeding 0.15°C have been used in constructing the contours.

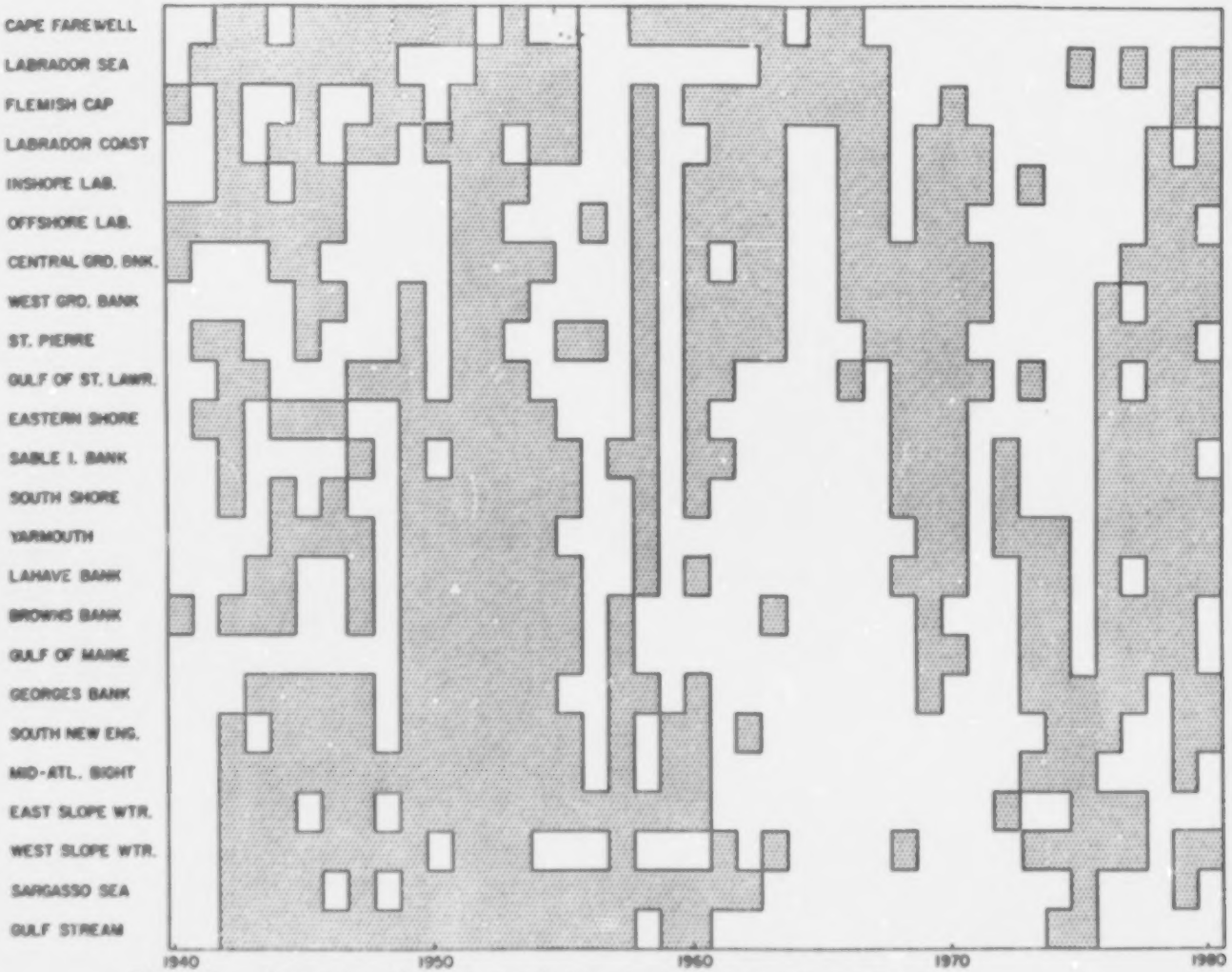

Figure 9 Space-time plot of annual SST anomalies over the period 1940-1980. Stipled areas depict positive anomalies.

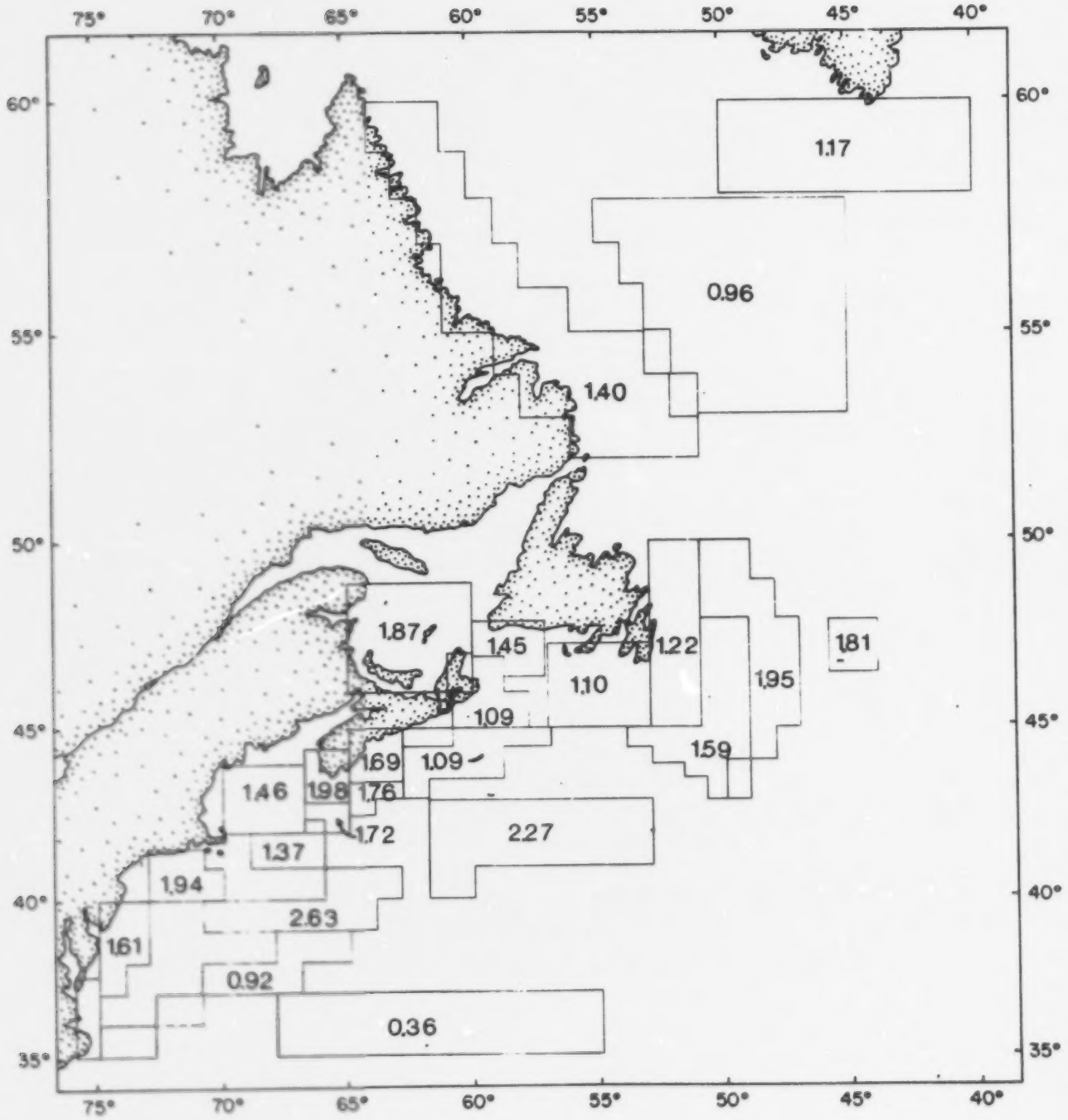

Figure 10 Chart of the areas for which SST's have been analyzed showing the variance of the particular SST signal (deseasonalized version) in Celsius degrees squared.

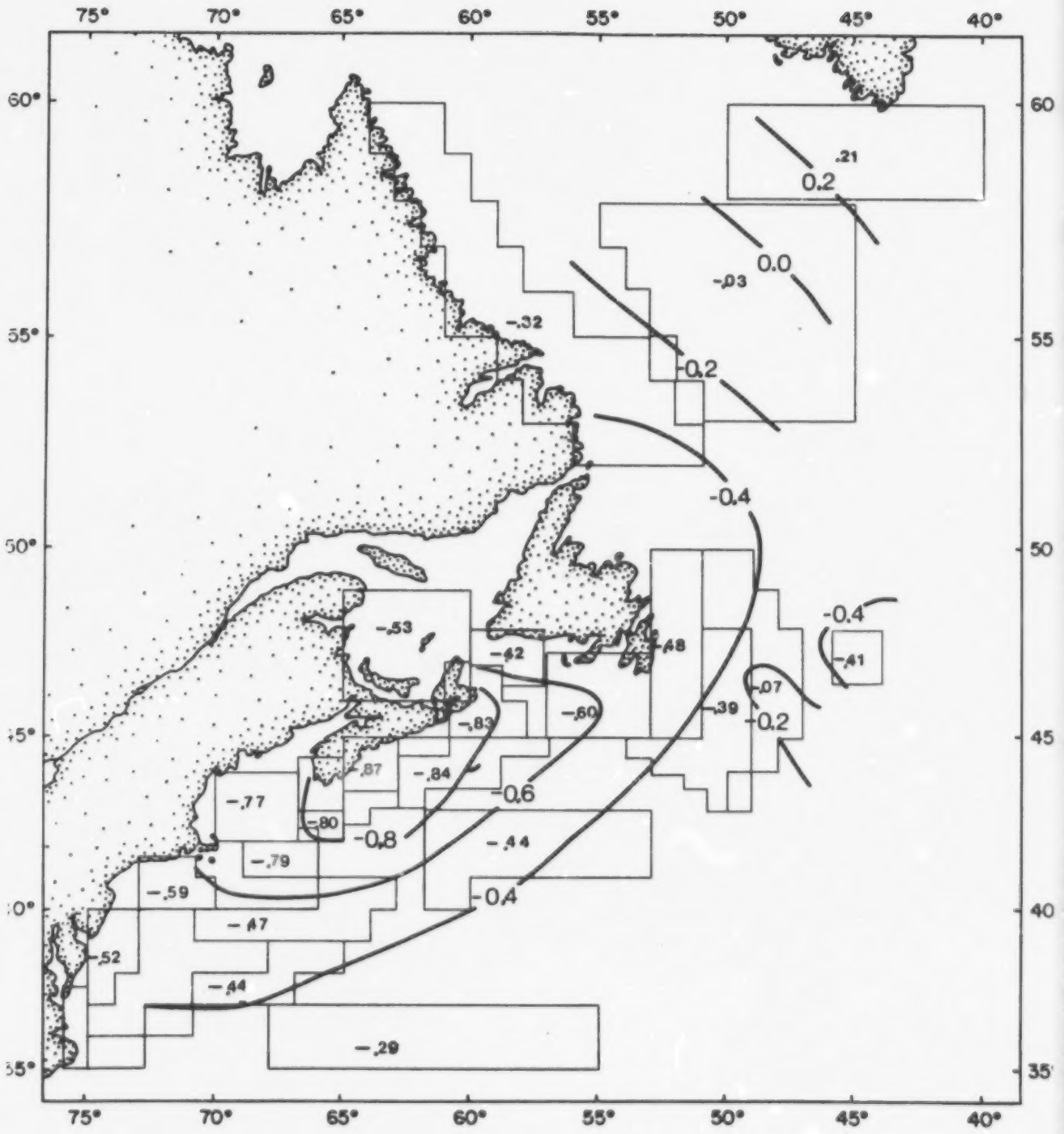

Figure 11 Chart showing eigenvector loadings for first mode EOF's of SST for winter.

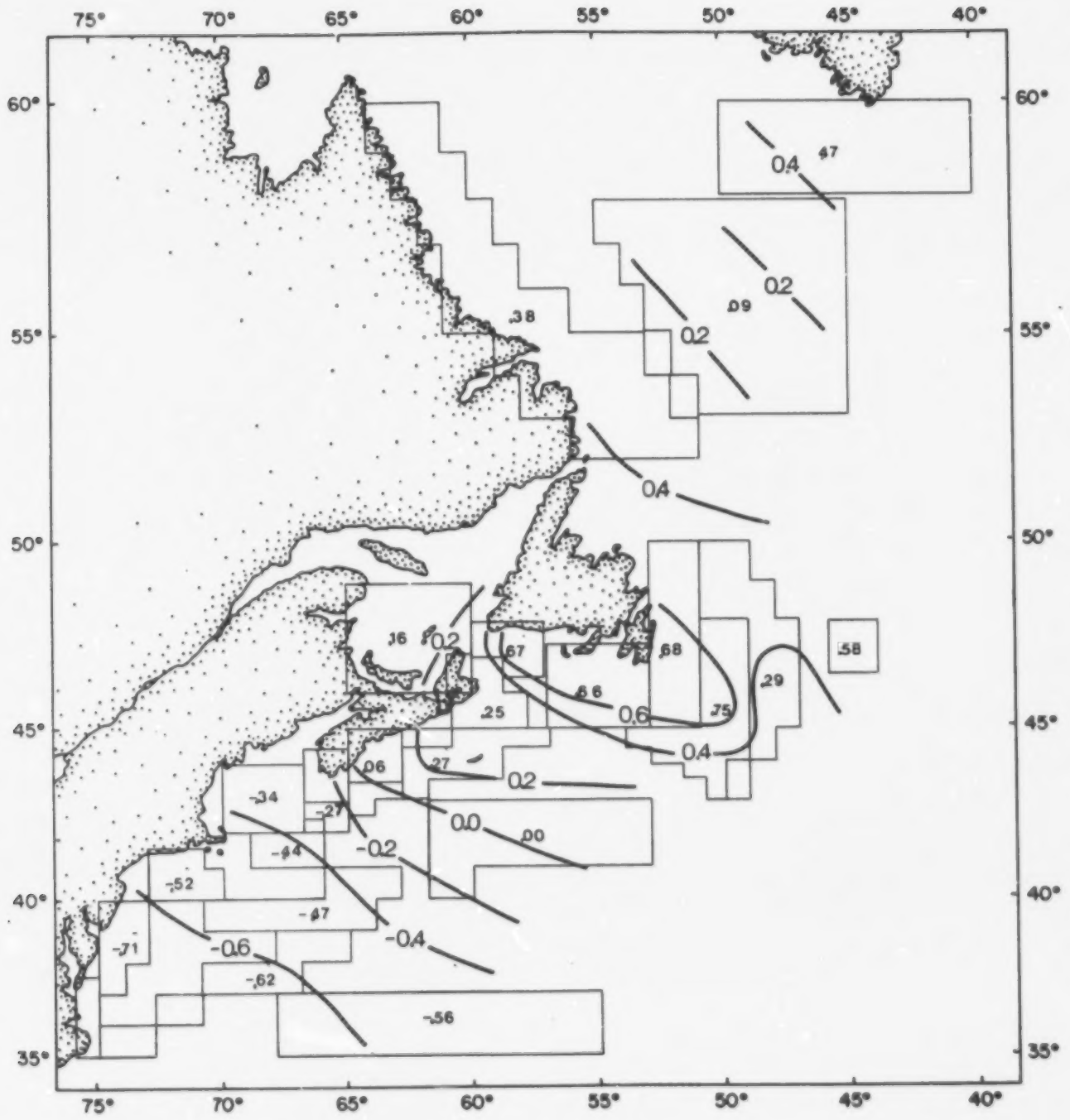

Figure 12 Chart showing eigenvector loadings for second mode EOF's of SST for winter.

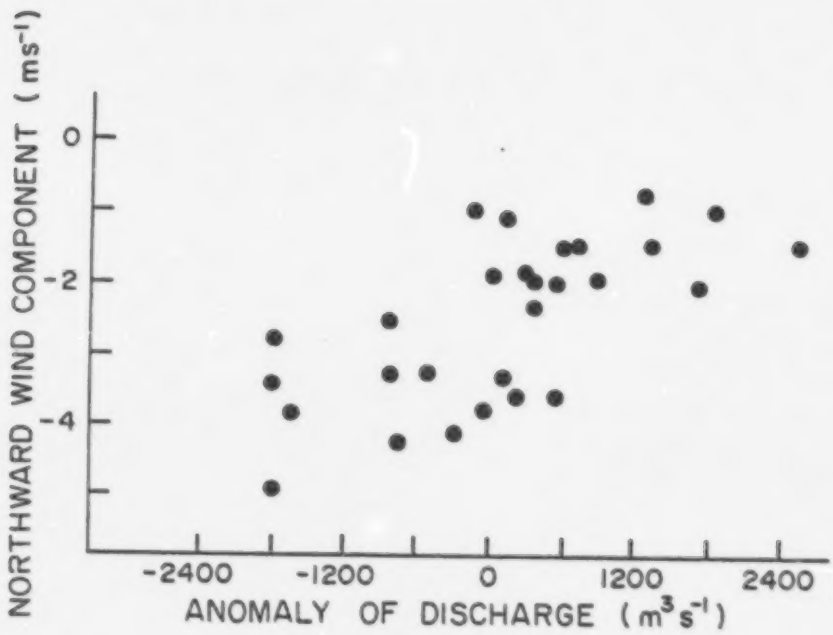

Figure 13 Scatter diagram of the north component of Scotian Shelf geostrophic winds (in winter) (Thompson and Hazen, 1983) and anomaly of annual discharge of the St. Lawrence Rivers (RIVSUM) (Sutcliffe et al., 1976) in that year.

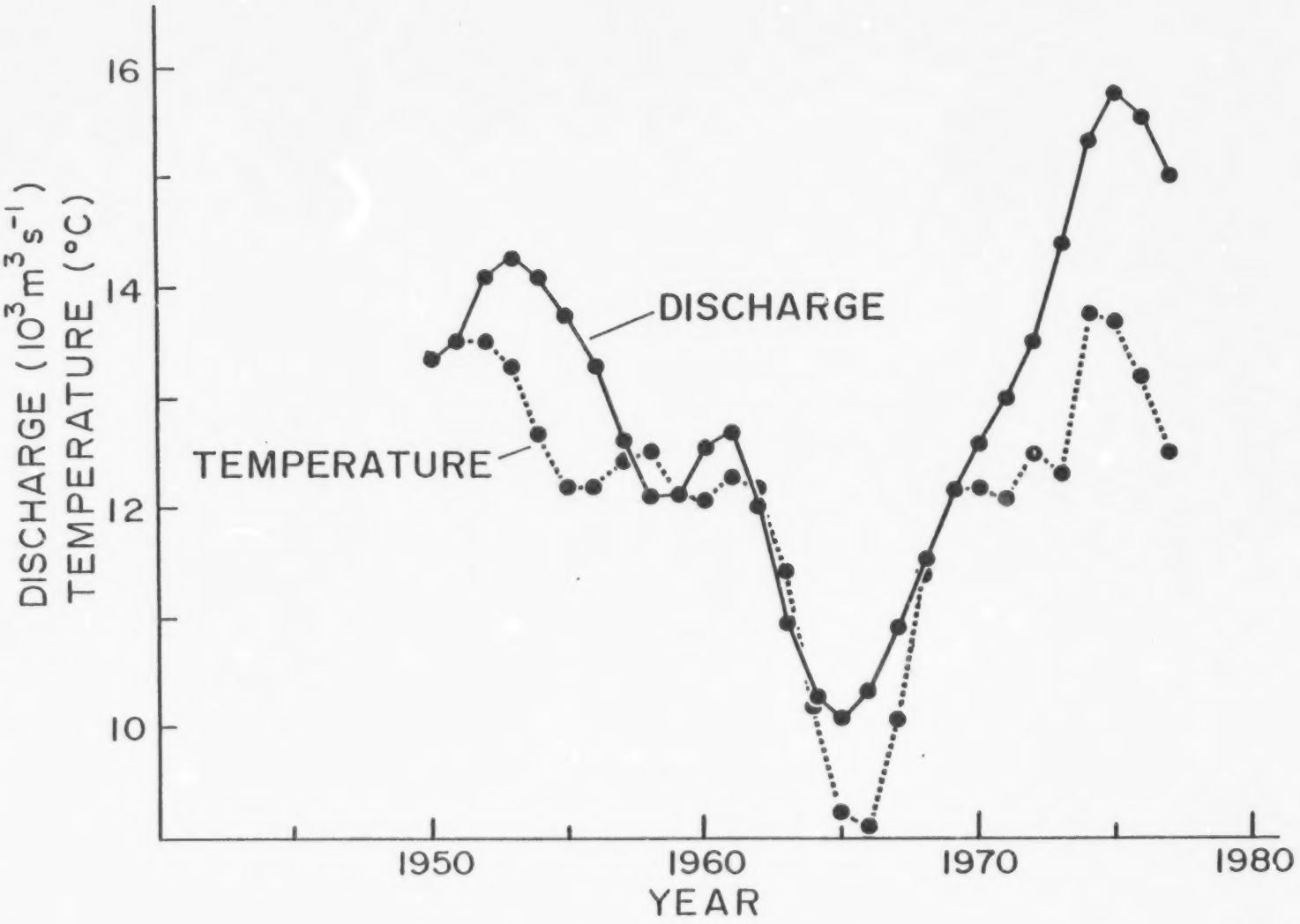

Figure 14 Plot of the St. Lawrence rivers' spring discharge and western Slope Water's SST the following winter using a 7 point normally-weighted running mean.

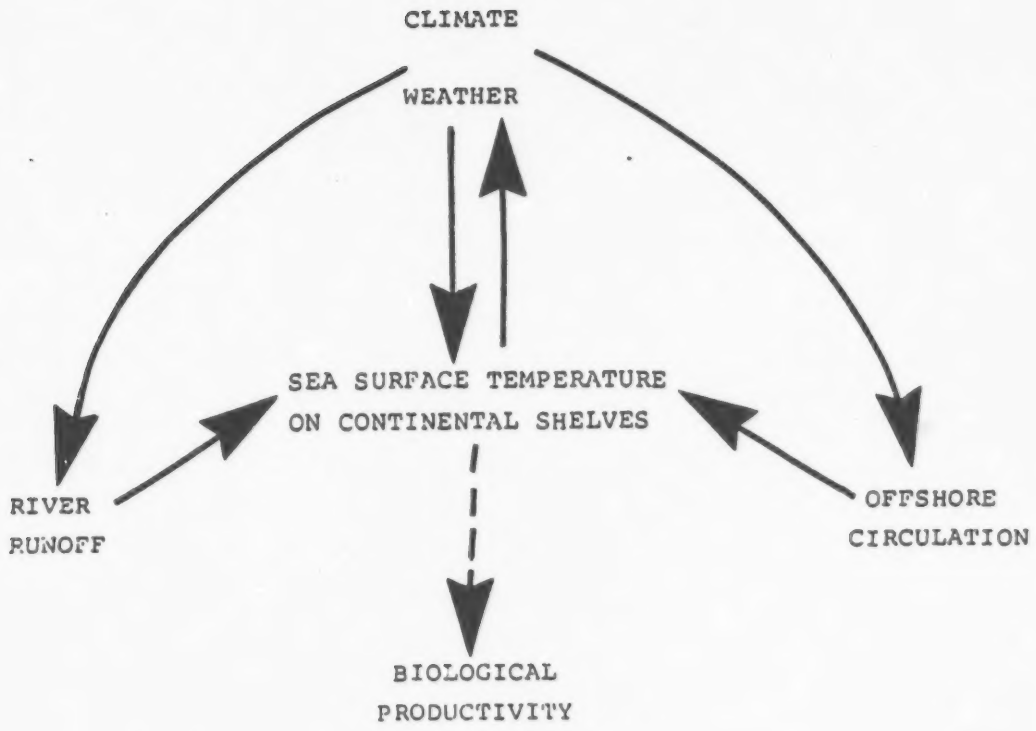

Figure 15 A schematic illustration showing that SST's are influenced directly and indirectly by meteorological forcing -- the indirect pathways involving either river runoff and coastal waters' vertical stability or the broad ocean gyres and the meanders and rings of the Gulf Stream.

Fishery Distribution on the Scotian Shelf

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Detailed set-by-set commercial fisheries data collected by fisheries observers on Canadian TC 4-6 otter trawlers operating on the Scotian Shelf and Georges Bank in 1982-1983 were analysed with cluster and discriminant techniques to define individual fishery units suitable for management purposes. In separate yearly analyses strong similarities in catch composition and location of the major fisheries were found. The Scotian Shelf fisheries were dominated by single-species fisheries for cod, haddock, redfish and pollock. The northeast portion of Georges Bank was found to support a mixed cod, haddock, and pollock fishery while a mixed plaice, yellowtail and cod fishery was found on the eastern Scotian Shelf. The commercial fishery catches resembled underlying species assemblages in both species composition and location.

Introduction

Most groundfish fisheries, especially otter trawl fisheries, yield catch from more than one stock at a time. This is a class of multispecies interactions referred to as technological interactions which are mediated directly through fishing effort causing mortality on more than one stock at a time (Anderson 1975). Technological interactons may also lead to sub-optimal mortality levels on the stocks involved (Paulik et al. 1967) or may lead to substantial losses in yield under single species quota management. To be effective fishery management regimes must consider these interactions. The logical first step is to develop robust definitions of individual fishery units.

The existence of distinct species assemblages of demersal fish on continental shelves has been demonstrated (Gabriel and Tyler 1980, Overholtz 1982, Mahon 1985). Individual fish species exist in discrete populations. Population density varies through time and space and populations of different species overlap to varying degrees through time and space. Fishermen, on the other hand, exploit these assemblages with the objective of maximizing catch per unit effort. They will go to specific locations where high catch rates of certain species are expected, and if these expectations are not met they move to other locations. Therefore, as a working hypothesis, it should be possible to define individual fishery units as concentrations of fishing effort which yield catches of unique, homogeneous species composition and which exhibit stability in composition and location relative to the underlying assemblage structure. Fisheries defined in this way would be suitable analytical units for investigating multispecies management options (Murawski et al. 1983a). The objective of this paper is to define the major fisheries exploited by Canadian offshore (Tonnage Class 4-6) otter trawlers on the Scotian Shelf and Georges Bank in 1982-83.

Cluster analysis has been used extensively to group site data based on similarities in species compositions (Clifford and Stevenson 1975, Gabriel and Tyler 1980, Overholtz 1982, Murawski et al. 1983a, Mahon 1985). A discussion of the utility of cluster analysis in defining fisheries management units has been prepared for MEES by Gabriel and Murawski (1985). Two important points about the technique are worth emphasining here. Firstly, the researcher may select the scale of fishery definition appropriate for the particular management situation since there are no hard and fast rules for determining when to stop clustering groups. However, this characteristic and the existence of a vast array of methods of cluster analysis may lead two researchers to completely different solutions when analysing the same data set. Consequently the interpretations of results of cluster analysis must be accompanied by other substantive analysis.

Methods

Detailed set-by-set species catch composition (weight) and fishing efforts (hours) for Canadian TC 4-6 otter trawlers for the years 1982 and 1983 fishing on the Scotian Shelf and Georges Bank were obtained from the Scotia-Fundy Region International Observer Program (Kulka and Waldron 1983). Only sets with complete position and time information and with no gear damage were selected. A total of 6,195 individual sets met these criteria. Given the large sample size some data reduction was required in preparation for cluster analysis.

The catches of 7 species were examined; cod, (Gadus morhua), haddock (Melanogrammus aeglefinus), redfish (Sebastes sp.), pollock (Pollachius virens), American plaice (Hippoglossoides platessoides), yellowtail (Limanda ferruginea), and witch (Glyptocephalus cynoglossus). These are the seven groundfish species fished by Canadians and regulated by quota in the area. Catch and effort data were aggregated by 10' squares of latitute and longitude by month and catch rates of the seven species were calculated. Monthly plots of catch

rates were examined to determine whether further aggregation by time period was appropriate. This revealed that four time periods could be used without masking seasonal trends, namely December-February, March-May, June-August, and September-November.

Separate cluster analyses were performed for 1982 and 1983 data. In 1982 the first season was only January-February and the data for December 1983 were not used. The cases used were determined as seasonal catch composition (% of total weight) by 10' square. Mean set start depth was also calculated for each case. By using a small geographic scale it was expected that depth variations among sets within cases would be minimized.

The CLUSTAN analysis package was used for cluster analysis (Wishart 1978). The Bray-Curtis dissimilarity coefficient was used in the procedure RELOCATE. This procedure began with an initial allocation of cases to 10 clusters followed by a series of scans where each case was considered in turn and its similarity with each individual cluster computed. If a case was more similar to another cluster than it's own, it was relocated. The centroids of the clusters were recalculated to account for this change at the time of the switch. The population was repeatedly scanned until no objects were relocated or until a maximum number of iterations had been carried out. Once a local optimum was established the two most similar clusters were fused and the cycle repeated. To test for a suitable solution three radically different starting allocations were selected. At each cycle the results of the three analyses were tested for convergence. Convergence was taken to suggest that a global solution had been obtained.

Following cluster analysis, case labels were plotted on a map of the study area. In this way the spatial homogeneity of the clusters, and thus the fisheries, could be examined. Comparisons were also made between the two yearly analyses to examine stability in catch composition and location.

Following the definition of fisheries using cluster analysis the relationship between these fisheries and the physical variables latitude, longitude, depth, season, and year was investigated using discriminant analysis (Nie et al. 1975). Three separate analyses were performed and compared, one for 1982, one for 1983, and a third for the years combined.

Results

There were 567 and 632 cases in the 1982 and 1983 analyses respectively. Using three different starting allocations of clusters there was complete convergence in both years at cycle 6 when 5 clusters remained. In the 1983 analysis convergence between 2 different starting allocations occurred at cycle 3, whereas in the 1982 analysis the same 2 starting allocations converged at cycle 5. Although convergence was not complete at cycle 4, the differences involved less than 10 cases. Cycle 4, with 7 clusters, gave an acceptable degree of convergence.

The characteristics of the fisheries defined by cluster analysis are listed in Table 1. The percent compositions of the various fisheries are plotted in Figure 1. The results were very similar between the two separate analyses even though they were performed independently. In both, there was 1 fishery with very little catch or effort distributed over two squares in 1982 and seven in 1983. These fisheries were indicative of very poor fishing conditions and were not considered viable. In both analyses there were four single species fisheries dominated by cod, haddock, redfish and pollock respectively. In each more than 80% of the total catch was of one species. Both analyses had a mixed plaice, yellowtail, and cod fishery. This will be referred to as the flounder fishery. In 1982 a mixed cod, haddock and pollock fishery was identified whereas in 1983 a mixed cod, haddock, redfish and pollock fishery was identified.

Cluster labels were plotted to investigate the spatial homogeneity of the results (Figures 2 and 3). Adjacent squares of the same fishery were contoured to emphasize spatial patterns. A map of the study area with the common fishing banks is given in Figure 4.

The cod fishery dominated depths less than 200 m along the edge of the Laurentian Channel, Banquereau Bank, Middle Bank and eastern Sable Island Bank. Seasonally the fishery varied considerably. In the winter months the fishery was concentrated along the edge of the Laurentian Channel. Cod fishing was also observed along the northern portion of Banquereau and Middle Banks. In March-May the cod fishery occupied the eastern and western sections of Banquereau but did not occur in the central portion. In 1983 this spring fishery also extended to the areas south of Sable Island. In the summer (June-August) the cod fishery was less evident but in 1983 it was observed on the southeast tip of Banquereau. In the fall the cod fishery in the area was concentrated in the eastern shoal area of Banquereau.

The cod fishery was not evident on the central or western Scotian Shelf in any season, but it did occur on Georges Bank. Whereas, there were relatively few squares classified as a cod fishery, they generally occurred along the western portion of the northern edge of the Bank in depths less than 200 m and in the period of June to November.

The haddock fishery was found to occur mainly on the central and western Scotian Shelf on Western, Emerald, Baccaro and Browns Banks. There were also some cases on Georges Bank. The fishery was strongly seasonal being a major fishery during March-May on Western and Emerald Banks, and to a lesser extent on the western Scotian Shelf in December-February. Most of this is closed to fishing by otter trawlers during the months of March-May. In the period June to November the haddock fishery was spread diffusely throughout the Scotian Shelf. On Georges Bank the fishery

was found along the northeast peak and on the southeast edge of the Bank generally in deeper water than either the cod or pollock fisheries.

The redfish fishery occurred at depths greater than 200 m along the edge of the Scotian Shelf and the edge of the Laurentian Channel. On the Shelf, pockets of redfish fishing could be found for the most part at depths greater than 150 m. This fishery was carried out almost exclusively in the months of June to November and no cases were found on or around Georges Bank.

The pollock fishery was found mainly on the central and western Scotian shelf and Georges Bank although there were scattered cases on the eastern Scotian Shelf. In March-May of both years the pollock fishery was carried out between and to the south of Emerald and LaHave Basins. During December-February there was also a pollock fishery on Emerald Bank. It is interesting that in March-May of 1983 the pollock fishery was replaced by the haddock fishery in virtually the same place on Emerald Bank.

The flounder fishery was relatively small and was found almost exclusively on Banquereau Bank in close proximity to the cod fishery. In some squares both the flounder and cod fisheries occurred but at different times. For the most part the flounder fishery cases were found on the perimeter of the major cod fisheries.

In both years other mixed fisheries were found but the species compositions of the fisheries varied significantly between years. In 1982 the fishery was dominated by cod, haddock and pollock species. The fishery consisted of 284 sets spread over 54 squares, 184 of the sets and squares were on Georges Bank intermixed with the individual cod, haddock, and pollock fishery cases. This high level of effort indicates that this mixed fishery was commercially viable. In 1983 the mixed fishery was less active with 29 sets in 19 squares. The catch composition was dominated by four species; cod, haddock, redfish, and pollock. The individual cases were spread throughout the study area. Given this low level of fishing activity and the lack of spatial homogeneity it is not likely that this was actually a viable individual fishery.

The results of the three discriminant analyses are given in Table 2. In all 3 analyses more than 95% of the model variance was explained by the first two discriminant functions. Consequently only these were used for classification.

The standardized discriminant function coefficients indicated a similar ranking of factor importance in the 1982 and 1983 analyses. In the first discriminant function depth followed by season accounted for over 75% of the variance. In the second function longitude then latitude were the dominant variables. When all the data were used in a third analysis a fifth variable, year, was introduced. The same pattern in standardized coefficient magnitude resulted with the first function being dominated by depth and the second by longitude. The variable year made an insignificant contribution. Since the results in all three analyses were similar only the combined analysis will be discussed further.

A plot of discriminant functions 1 and 2 is given in Figure 5 with the axis of the 5 variables used indicated. Clearly the redfish fishery (symbol 3) is separated from the others along the depth axis. Furthermore, the cod (1) and flounder (6) fisheries dominated the lower longitudes. This corresponds to the eastern Scotian Shelf area. However there were some cod cases in the higher longitudes, on Georges Bank. The haddock (2), pollock (4) and two mixed fisheries (7 and 8) are found intermixed in low depths and mid to high longitudes.

The overall level of correct classification resulting from the analysis was low at 44.7% (Table 3). The redfish fishery was the best classified at 75.4% due to the extreme depth of this fishery. There was a high degree of interclassification between the cod and flounder fisheries with 47% of the cod fishery cases being classified as flounder and 25% of the flounder fishery cases being classified as cod. However, for both fisheries 89% of their respective cases were classified as being either cod or flounder. This may be expected when one considers that together the cod and flounder fisheries by far dominated the shallow regions of the eastern Scotian Shelf.

The remaining four fisheries, haddock, pollock and the two mixed fisheries, were poorly classified by the physical variables. Both the haddock and pollock fisheries had many cases classified as mixed fisheries and the pollock fishery had the lowest percentage correctly classified.

It was interesting to compare the discriminant function plots of the cod and haddock fishery cases (Figures 5 and 6). While for the entire analysis the level of correct classification was poor these two fisheries appeared in different portions of the plots. In the lower longitudes (Eastern Scotian Shelf) the cod fishery dominated and occupied a wide range of depths. In the higher longitudes (Georges Bank) the cases were at shallower depths. The haddock fishery, on the other hand, occurred from the mid Scotian Shelf to Georges Bank and there appeared to be a trend of increasing depth in a westerly direction. On Georges Bank the cod and haddock fisheries appeared to be separated by depth.

Discussion

The analysis indicated the predominance of single species fisheries for this fleet of vessels directed for cod, haddock, redfish, and pollock. These fisheries were more heavily fished, had more consistent species compositions, and, with the exception of Georges Bank, had a higher degree of spatial homogeneity than the mixed fisheries. This may reflect the ability of the fishermen to select areas of pure catch within assemblage boundaries rather than reflecting the true assemblage structure. In addition the smaller

species which would be present in an analysis of assemblage structure using research survey results would not appear in the commercial catches due to selectivity of the larger meshed commercial gear.

The mixed fishery cases from the 1982 analysis were found mainly on Georges Bank intermixed with the individual cod, haddock, and pollock fishery cases. Murawski et al. (1983a) identified this as a mixed fishery area for cod (45%), haddock (30%), winter flounder (Pseudopleuronectes americanus) (6%), yellowtail (5%) and pollock (5%) from an analysis of U.S. commercial statistics from 1977 to 1978. Catches of flatfish species by Canadian vessels in this area have traditionally been very low. Given the relatively poor spatial separation of the individual fisheries on Georges Bank it may be most appropriate, for management purposes, to classify all fishing in this area by Canadian offshore vessels as mixed cod, haddock, and pollock.

The mixed cod, haddock, redfish and pollock fishery identified in the 1983 analysis had relatively few sets and a low overall catch rate. As such it is not likely that this was a viable commercial fishery.

Seasonal patterns were apparent in the analysis. In the months of December-May the shallow water fisheries on the Scotian Shelf were highly aggregated. In the summer months these same fisheries were not as well defined possibly indicating the dispersion of schools until densities were not commercially attractive. At the same time fishing on Georges Bank and in the deep areas along the Shelf edge was heavy. In the fall the fisheries on the eastern Scotian Shelf began to reform while fishing in deep areas and on Georges Bank continued.

The discriminant analysis indicated that depth followed by longitude were the most important of the physical parameters investigated. The redfish fishery was separated from the others along the depth axis, whereas the cod and flounder fisheries were separated from the other shallow water fisheries along the longitude axis. However, there was low overall discriminating power associated with the physical parameters. This may be due in part to the method of classifying the cases rather than to high variance in the data. The discriminating function is a linear combination of parameter values. However the distributions of some of the fisheries along the major axes, depth and longitude, were bimodal. For example, the cod fishery was found at the ends of longitude axis but not in the middle. Consequently, while the geographic locations of the individual fisheries may be stable from year to year the discriminant analysis is not very effective at predicting the type of fishery given the same physical data.

The results of this analysis generally supported the hypothesis that individual fishery units could be defined based on similarities in catch composition and that the catches would reflect stability in composition and location relative to the underlying assemblage structure. There were considerable similarities in catch composition of the major fisheries between years (Figure 1) and there was a high degree of spatial homogeneity of the fishery cases, both within seasons and between years (Figures 2 and 3). By using a small geographic unit for data aggregation, the 10 minute square, these boundaries were defined on a fine scale. These results agree with the assemblage structure of the Scotian Shelf proposed by Mahon (1985) in terms of both composition and location. In his study redfish were found in a species group which occupied the Shelf edge and the deeper waters of the central Shelf. On the eastern Scotian Shelf a cod, plaice, thorny skate group was identified while haddock was found in a group which occupied the western and central Scotian Shelf. He also found a high degree of consistency among years in the spatial aggregation of individual sets, and species groupings which were similar from year to year. Given this apparent stability of the Scotian Shelf assemblages and the precision of set-by-set commercial data available from the International Observer Program the composition and location of major commercial groundfish fisheries in the area should be predictable.

Fishery units defined on a fine spatial scale and on the basis of homogeneity of catch composition have definite advantages for management. While management objectives may be oriented toward individual species the desired effects can only be attained by controlling the relevant fisheries. Management measures such as catch quotas, by-catch limits, closed areas, and seasons are sensitive to variations in species composition and the location of the target fisheries. Linear programming analyses of technological interactions require input data which accurately reflect the species composistions of individual fisheries (Murawski et al. 1983b). From the perspective of fisheries monitoring this analysis indicates that it may be possible to identify the specific fishery a vessel is engaged in based only on the location of the vessel. Considering these factors fishery definition becomes the logical first step in fisheries management.

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Discussion Period

O'Boyle: How many different ports do the vessels sail from and if you look at the different fleets do you find them doing different things, for example, the Canso fleet as compared to the Lunenburg fleet?

Sinclair: There are about five ports and three companies involved. In general the area fished on the Scotian Shelf is not related to the port of sailing for this type of vessel. For the smaller vessels, less than 65', it is a different story.

Murawski: I couldn't resist the temptation to take your map from the summer period and put it alongside the map from Mahon's paper. There are some interesting similarities. If you filter out the noncommercial species, there are similar site groups. Particularly the redfish complex which lies along the shelf edge and comes in the middle, deeper, part of the shelf. What is not defined very well in the commercial data is the cod, thorny skate, plaice complex off the northeastern shelf because there were no fishing trips there in that period. However, there are some similarities which begs the question as to whether there are some complexes of interacting species which are exploited as such.

Sinclair - The analysis of commercial catches had a number of differences from the analysis of survey data. The species used in this analysis were those for which there are catch quotas. Therefore, species such as skates and sculpins were not used and the fisheries species compositions were much purer than the assemblage compositions. Also the commercial gear does not select the smaller individuals, effectively removing them from the catch. The fishermen also seem to be able to select areas of relatively pure catch. So, in answer to your question, for the most part the fisheries I have defined seem to be single species fisheries. Two mixed fisheries were defined, one for flounder and cod, and one for cod, haddock and pollock. These species would then be involved in significant technological interactions. That leaves the question of biological interactions which were not really studied here.

Murawski: Another question is what we would see if there were survey data on a seasonal basis rather than just for the summer.

Mahon: A major discrepency is in the haddock fishery in the middle of the shelf. This is a winter/spring fishery and we do not see it in the summer survey since by that time the haddock are more spread out.

Rice: I agree that your discriminant analysis may not be the appropriate tool. However, it would be very useful to predict what the fishery is going to encounter in a given area based on characteristics of a place, such as water temperature and depth. It might be more useful to discriminate between areas of low or no abundance and areas of high abundance for each fishery rather than to attempt to discriminate between fisheries which have been defined after the fact.

Sinclair: Often when we analyse catch and effort data we face the difficulty of defining what the directed species of the trip was. We do this basically by looking at the catch. When in fact the fishermen, based on experience, go to areas they know will give them what they want.

Rice: So it is not a difference between cod and haddock and pollock. It is a difference between cod and where they are not or haddock and where they are not. Analysing it that way could make the problem a lot more tractable.

Sandeman: An important point here is that these are large vessels fishing for specific markets.

Sinclair: Yes, their fishing strategies are often very species specific, often dictated by the company.

McKone: They are even in radio contact and the company could be controlling the catch composition on a daily basis.

Kerr: On a different topic, is there a difference in temperature between that eastern and western line that separates cod and haddock?

Mahon: Yes, there are differences in temperature and salinity and the details of this are discussed in the paper by Mahon and Sandeman.

Table 1. Characteristics of the fisheries defined by cluster analysis of catch composition for 1982 and 1983 observed trips on Canadian TC 4-6 otter trawlers.

Fishery	No. Cases	No. Sets	Hours Fished	% Composition							Total Catch (t)
				Cod	Had	Red	Pok	Pla	Yel	Wit	
1982											
Cod	181	1165	2275	85.6	3.3	.7	3.3	1.6	1.3	.3	4015
Had	125	609	1460	6.6	85.5	1.3	3.7	.1	.1	.1	1501
Red	64	160	355	3.0	1.3	86.2	5.2	1.0	.3	.1	347
Pok	106	513	1091	5.5	6.0	2.2	85.1	0	0	0	1844
Flx	33	135	337	18.7	.7	1.0	.1	36.5	23.9	11.9	279
Mix	56	284	615	45.6	26.5	.2	21.8	.1	.1	.1	479
0	2	2	5	6.3	3.7	0	0	2.5	0	0	0
1983											
Cod	231	1635	3889	86.5	2.8	.5	3.0	1.2	1.6	.2	5110
Had	116	342	826	7.9	80.7	.6	4.9	.3	.2	.2	554
Red	147	495	1378	2.7	.5	91.3	1.7	.6	.1	.1	1104
Pok	81	414	951	5.9	2.9	1.4	88.2	.1	.1	0	1515
Flx	31	97	253	20.9	.3	.9	.5	28.3	13.1	2.2	151
Mix	19	29	59	10.7	23.1	32.1	12.2	1.3	.3	1.5	35
0	7	11	27	17.1	.5	0	1.3	1.1	0	8.6	13

Table 2. Results of discriminant analysis of cluster definition using physical parameters latitude, longitude, season, depth and year.

Variable	Standardized Discriminant Function Coefficients					
	1982		1983		Years Combined	
	Function 1	Function 2	Function 1	Function 2	Function 1	Function 2
Latitude	-.12	-.29	.05	-.24	-.02	-.25
Longitude	-.35	.75	.05	.82	-.12	.79
Season	.41	.10	.46	-.07	.43	.02
Depth	1.00	.06	.95	.09	.96	.10
Year					.10	-.12
Cumulative % of variance	69.04	97.74	79.71	98.79	73.89	96.28

Table 3. Classification results of discriminant analysis using latitude, longitude, depth, season and year.

Acutal Fishery	No. of Cases	Percent of Actual Fishery in Predicted Group						
		Cod	Had	Red	Pok	Flx	M82	M83
Cod	412	42.5	1.0	1.0	0	47.1	8.0	.5
Haddock	241	13.7	42.3	0	7.1	1.7	23.2	12.0
Redfish	211	.9	0	75.4	4.3	8.5	1.9	9.0
Pollock	187	7.5	18.7	2.1	16.0	9.6	26.7	19.3
Flounders	64	25.0	4.7	3.1	3.1	64.1	0	0
Mixed 82	56	12.5	19.6	0	5.4	1.8	35.7	25.0
Mixed 83	19	5.3	0	10.5	0	15.8	42.1	26.3

Overall Correct Classificaiton = 44.7

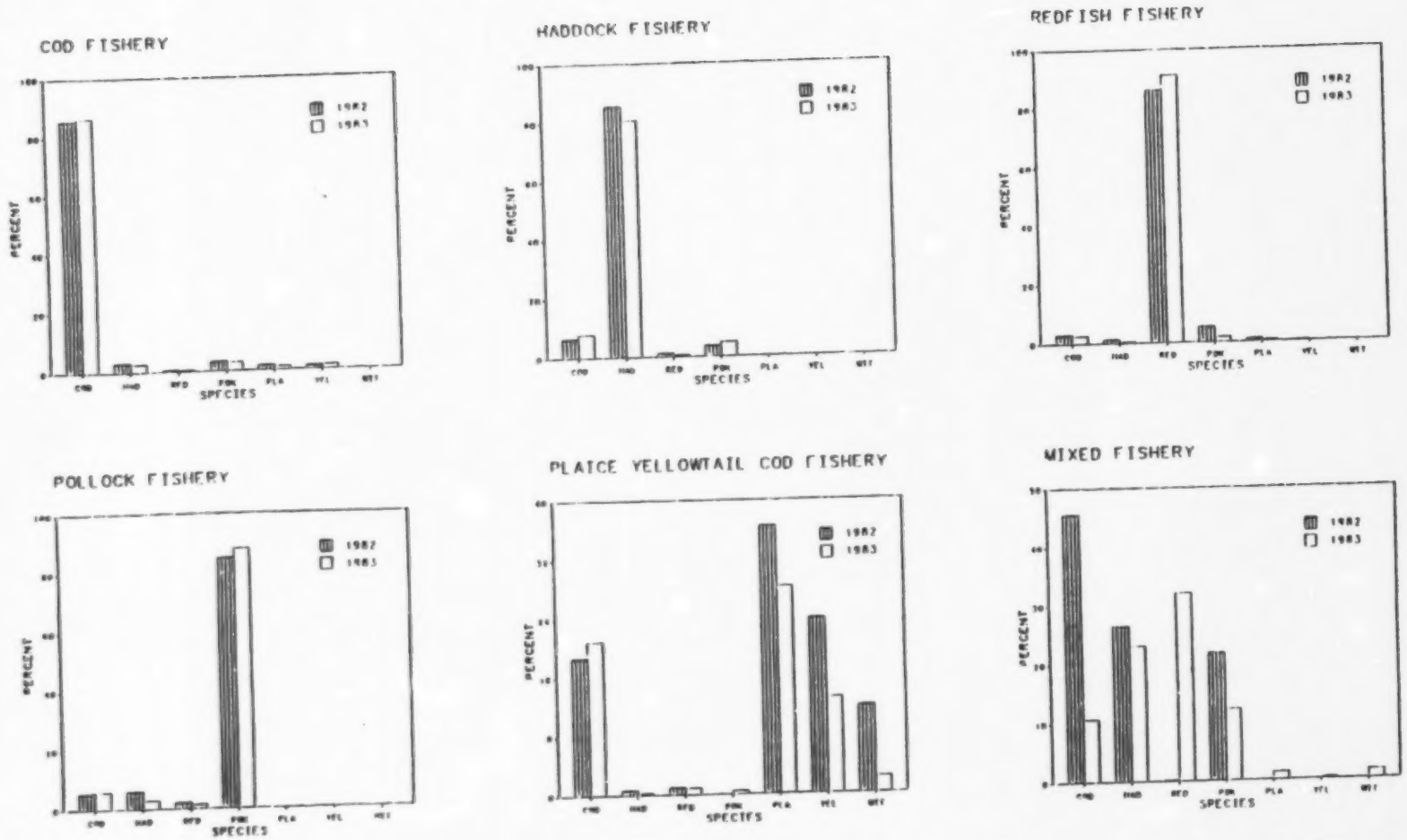

Figure 1. Species compositions of the fisheries identified by cluster analysis of 1982 and 1983 observer data from Canadian TC4-6 otter trawlers.

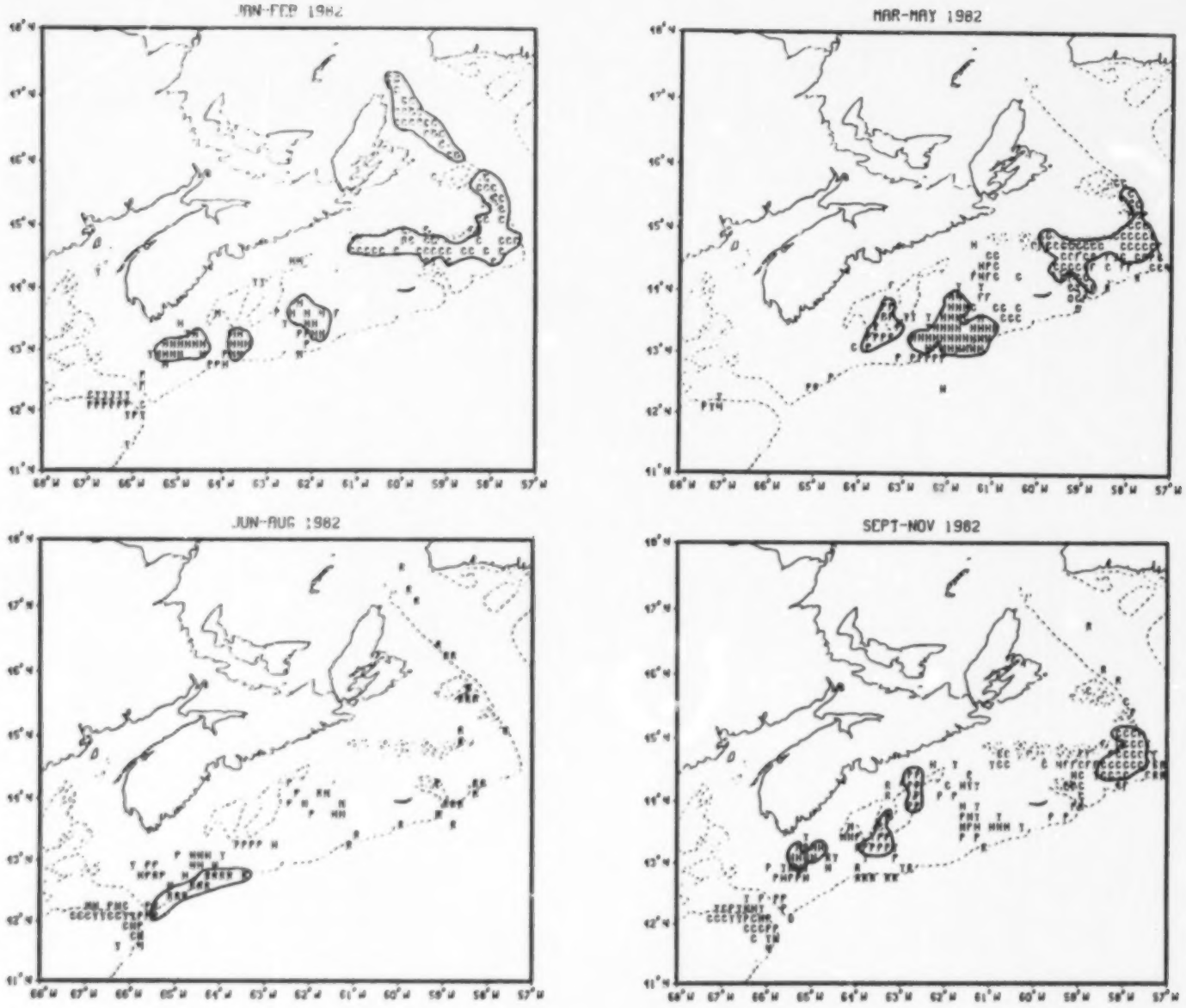

Figure 2. Plot of cluster labels resulting from the analysis of species composition in the catches of Canadian TC4-6 otter trawlers in 1982. C = cod, H = haddock, R = redfish, P = pollock, F = flounders, Y = mixed.

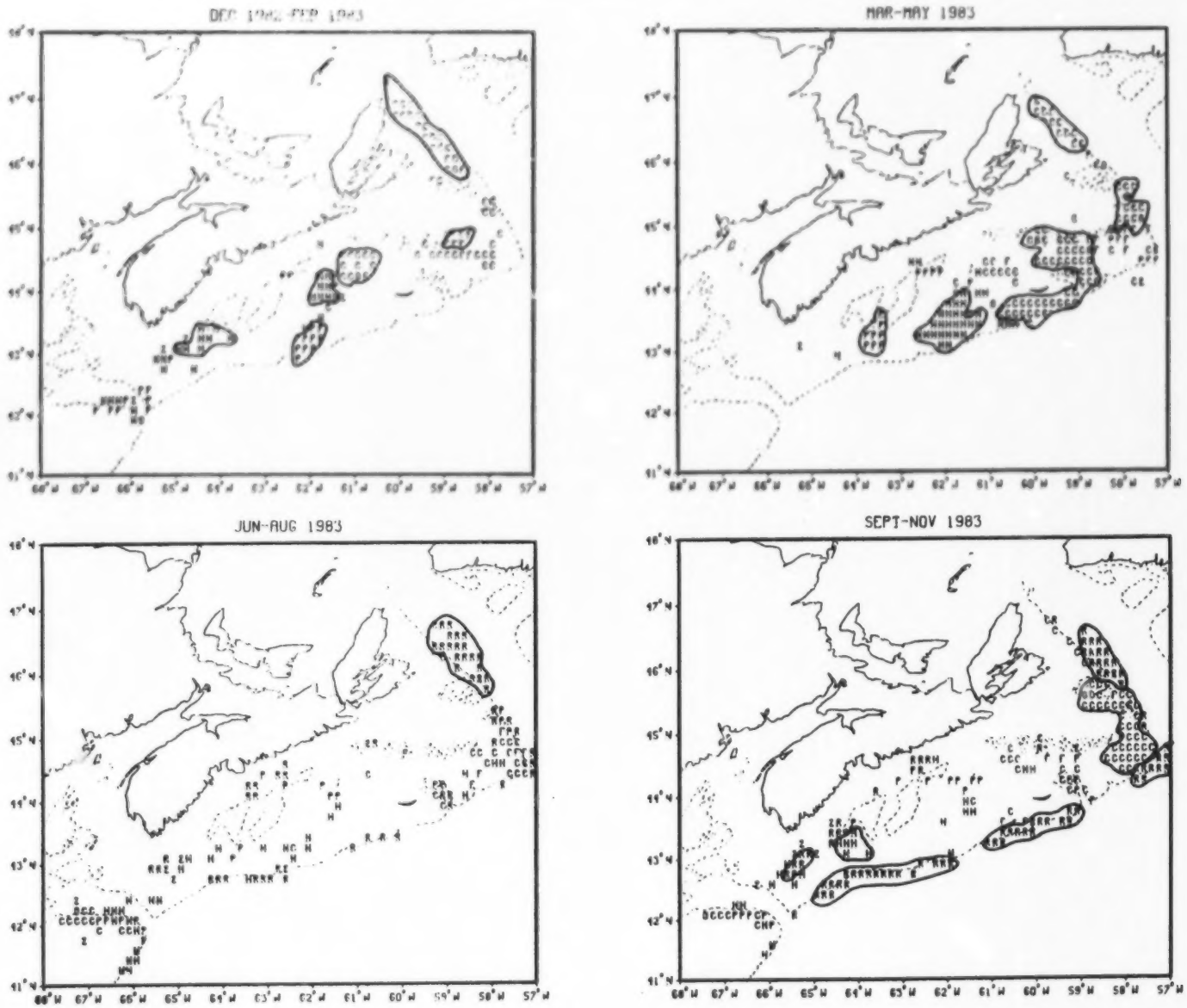

Figure 3. Plot of cluster labels resulting from the analysis of species composition in the catches of Canadian TC4-6 otter trawlers in 1983. C = cod, H = haddock, R = redfish, P = pollock, F = flounders, Z = mixed.

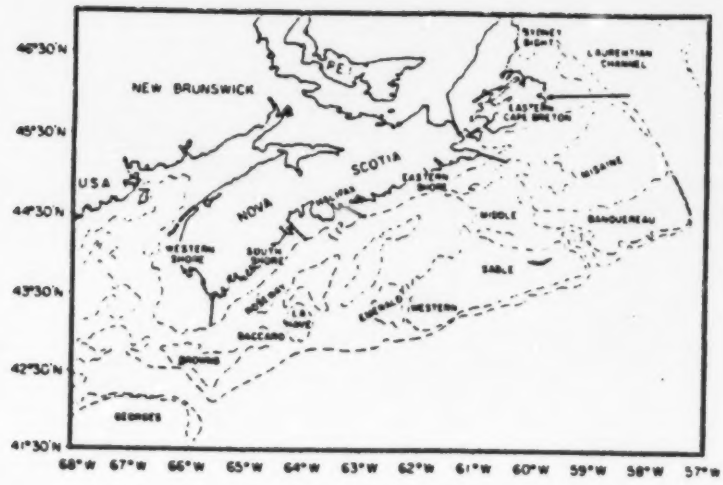

Figure 4. Map of the Scotian Shelf indicating common bottom features.

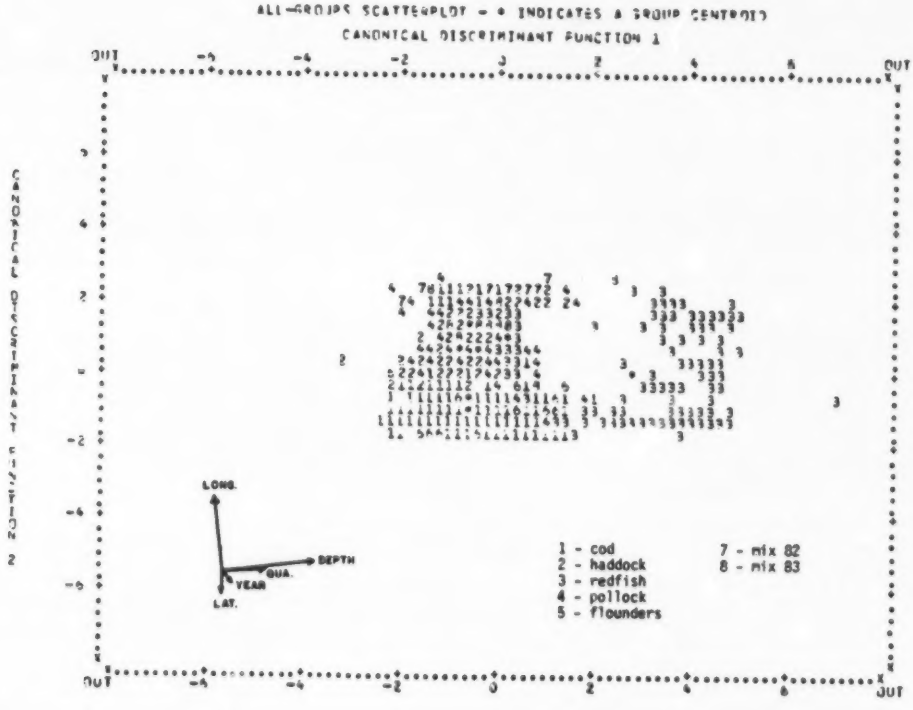

Figure 5. Discriminant function plot of all cases used in the combined years discriminant analysis.

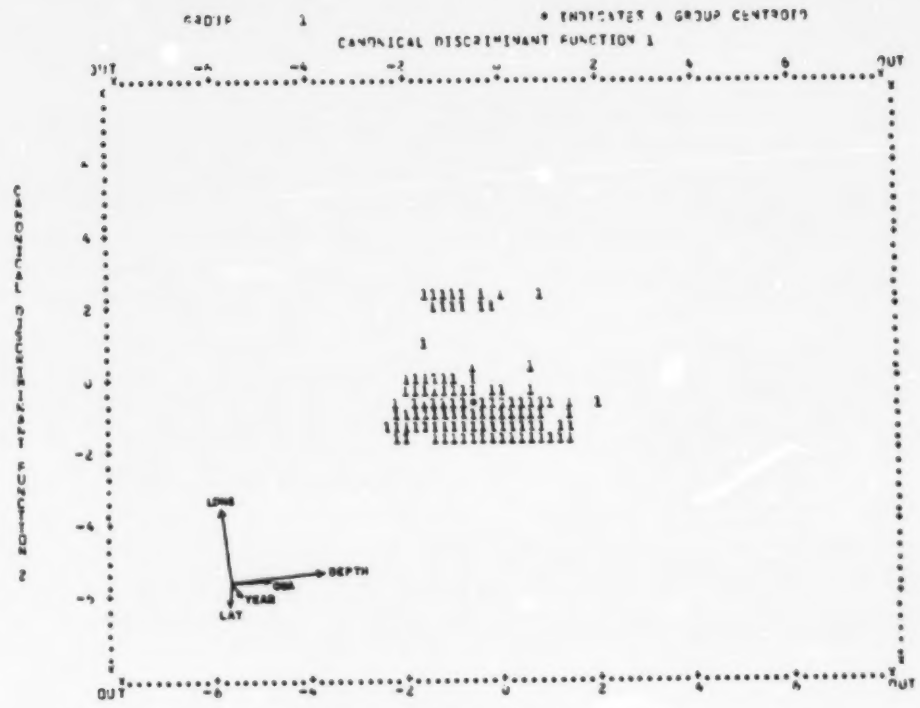

Figure 6. Discriminant function plot of only cod fishery cases.

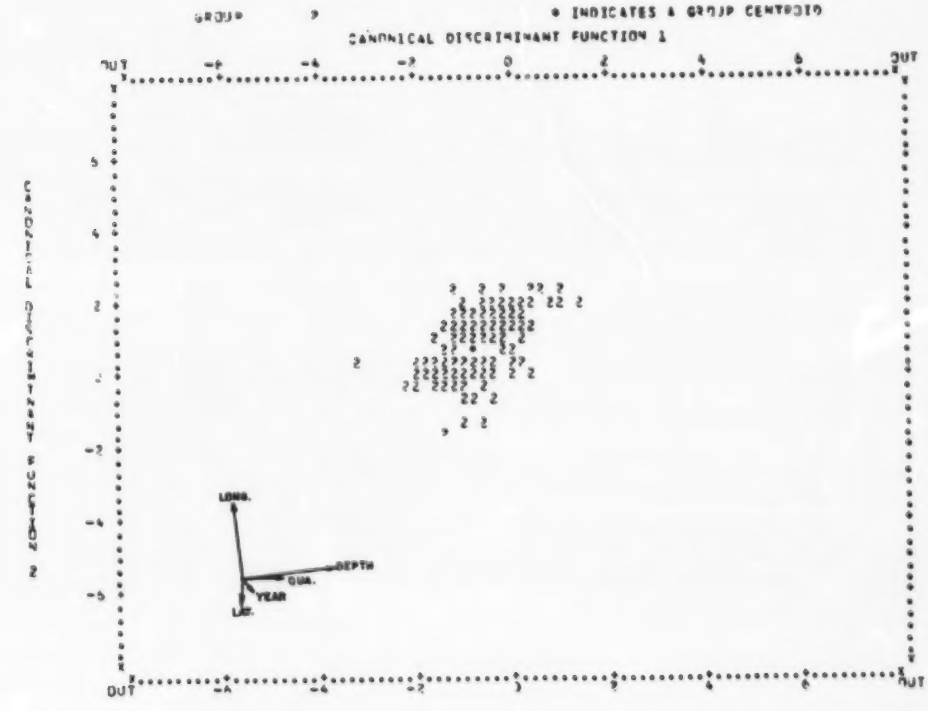

Figure 7. Discriminant function plot of only haddock fishery cases.

Recruitment Variability - Biologically or Environmentally Driven?

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Koslow, J.A. 1985. Recruitment Variability - Biologically or Environmentally Driven? p. 194-195. In: R. Mahon [ed.] Towards the Inclusion of Fishery Interactions in Management Advice. Can. Tech. Rep. Fish. Aquat. Sci. No. 1347.

I examined recruitment time series based upon VPA analyses for 20 stocks from the Labrador Sea to Georges Bank, 15 of which were cod, haddock, and herring. Correlation and principal component analyses (PCA) using the log-transformed data indicated recruitment was strongly correlated within species and, in cod and haddock in particular, on large spatial scales. There was no indication of heightened correlations (i.e. multi-species interactions) among stocks within localized areas. Temperature, salinity, and wind data were examined and similarly showed large-scale variability. Regression analyses indicated recruitment to cod, haddock, and herring stocks was generally positively related to salinity and negatively related to St. Lawrence River runoff. Recruitment to cod and haddock stocks tended to be significantly related to cool conditions around the Grand Banks and warm conditions from the Gulf of Maine south. Cod and herring recruitment was associated with southerly winds off the Scotian Shelf as well. The positive relationship between fishery recruitment and salinity is consistent with our present understanding of factors regulating productivity in northwest Atlantic shelf waters, whose primary sources of nutrient are from relatively saline water masses (e.g. the slope water). These results indicate large-scale climatic forcing predominantly regulates recruitment to most north-west Atlantic fish stocks, possibly through regulation of production at the base of the food chain. These results point to the possibility of modelling large-scale, low-frequency fluctuations in cod and haddock recruitment based upon regressions with dominant climatic signals. Further study is required to elucidate the mechanisms linking large-scale climatic signals and regional fishery recruitment.

Discussion Period

O'Boyle: What you are pointing out is that environment is the main signal affecting recruitment in the area, and other people have been saying that the major influence of species interactions on recruitment would be in the egg and larval phase. This is a key point since what Tony Koslow is saying is forget about multispecies interactions at this stage.

Zwanenburg: You lagged recruitment estimates from VPA to the year in which you have environmental data.

Koslow: Yes, I take the first available age from the VPA and lag it back to the year in which the cohort was spawned.

Zwanenburg: Doesn't this assume constant mortality from age 1-3?

Koslow: This approach assumes that events in the first year of life determine year class strength.

Zwanenburg: Couldn't that be introducing a lot of variance, introducing nonexisting relationships, or masking existing ones?

Koslow: It certainly could mask relationships. What is surprising is that such strong relationships do emerge. Clearly, there is a strong signal there.

Zwanenburg: My concern is that they may only appear strong and not be strong.

Koslow: It is not clear to me what particular bias might be introduced.

Mahon: I have another concern, where 4VW haddock is concerned the big dip in recruitment in the seventies corresponds to a dip in stock abundance which we usually think of as having been caused by overfishing. Therefore, it seems that the decline in recruitment could have been caused by recruitment overfishing, and I suspect that a similar case could be made for other stocks, especially haddock. How do you deal with that possibility?

Koslow: I was concerned about that and looked for stock-recruitment relationships in 11 stocks for which there was stock spawning biomass data. For only one case was there a significant correlation. Even looking at the trends, some were negative. Basically, I do not find any evidence for stock recruitment relationships in these stocks. Furthermore, stocks on the other side of the Atlantic in the ICES area were being heavily fished at the same time. If we look at the pattern of declining recruitment here, it is virtually a mirror image of the high recruitment, known as the gadoid outburst, in the northeast Atlantic, which occurred in the face of low stock size. One could speculate that opposite environmental effects were the cause of this. During

that period, an atmospheric low over Greenland led to a series of warm winters here with high runoff and low salinity. On the other side of the Atlantic, the low led to predominantly northerly winds and produced the opposite effect. Also, as Halliday and Pinhorn pointed out yesterday, fishing effort remained high until about 1974 and then was reduced. In all the gadoid stocks we examined, recruitment declined until 1971 when the low salinity event reversed; recruitment then increased.

McKone: You looked at the VPA for 4VWX redfish and only one age-length key was used to construct that. Most age length keys for redfish are very poor at the younger end.

Koslow: Redfish should be eliminated then. Redfish are not a key element in this analysis, which is primarily concerned with the gadids. More generally, the fluctuations we are talking about are on a log scale, i.e., over several orders of magnitude. Therefore, the pattern remains despite small biases or errors in the data. When the VPAs are updated, the patterns do not vary significantly on the log scale.

Dickie: Even if the data were free from various kinds of error, I would take exception to your conclusion that there are no multispecies relationships. All the correlations and relationships we have been dealing with are annual or year-to-year relationships. Under ordinary circumstances, it seems that what we study are these shorter-term, higher frequency effects on year-class strength which will certainly account for most of the variance in the data and are correctly studied in relation to environmental effects. However, multispecies relations are in the class of density-dependent effects which would only show up as longer-term effects at lower frequencies. If one expects to find multispecies effects, one would have to deal with filtered data which would exhibit these lower frequencies.

Koslow: I do not say that there are no multispecies interactions -- only that they appear to be relatively unimportant compared with environmental effects. If there are density dependent interactions, they must be operating year-to-year as well as on longer time scales. Furthermore, most correlations I have shown have a strong low-frequency component -- for example, the trend in most regional gadoid stocks of high recruitment in the early 1960s followed by a steady decline until the early 1970s, which is in turn followed by generally increasing recruitment into the mid 1970s.

A Description of the Two-Tier Catch Quota System of ICNAF, with Commentary on its Potential Usefulness in Current Fisheries Management on the Scotian Shelf

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During 1974-76, ICNAF used a two-tier catch control system in Subareas 5 and 6 to take account of both biological and technological species interactions. This approach required the establishment of an overall TAC for the area which was generally 20 percent lower than the sum of the TACs for the individual stocks. The Schaefer model was used to define overall MSY levels while linear programming was employed to assist in the inter-country allocational process. The process was conservative and tailored to the existing management environment. Application of a second tier today would meet with resistance and be difficult and costly to enforce. On the other hand, linear programming techniques could be employed to assist in allocational issues among Canadian domestic fleets. Such an approach could include economic parameters in the analysis.

Introduction

Although there has been a great deal of research on multispecies theory, little of the results has been applied in real management situations. One exception to this is the management of fisheries in the Northwest Atlantic during 1974-76. Under the auspices of the International Commission for the Northwest Atlantic Fisheries (ICNAF), an extensive body of management related scientific knowledge was developed and applied to the finfish resources in Subareas 5 and 6 (Figure 1).

The system used was the so-called two-tier approach because it constrained not only the exploitation of each stock in the management area but also the exploitation of the entire ecosystem.

This note documents the theoretical and practical framework of this management approach and provides comments on how successful such an approach could be today.

History and Rationale

During the mid 1960s, the finfish stocks in most of the ICNAF area experienced heavy exploitation. Stock-specific Total Allowable Catches (TACs) were first applied to Division 4X and Subarea 5 haddock in 1970 in an attempt to curb the declines in stock size. Other species were brought under TAC regulation in subsequent years. It soon became evident, however, that further measures would be required to both conserve the resource and allow rebuilding. At the January 1973 meeting of ICNAF, the USA made a detailed proposal for Subareas 5 and 6, which specified fishery effort allocations by country. This proposal was intended to put an overall limit on exploitation in the area. No agreement could be reached on whether or not to use catch or effort limitations. However, the need for some form of overall regulation on the fishery was recognized.

At the June 1973 meeting, the USA again tabled the proposal, this time outlining both overall and stock-specific TACs. This was the first time that the two-tier approach was so named. It was adopted as a catch control management tool at a special meeting in the following October.

The stated rationale for its use were:

1. to compensate for by-catch mortality which is difficult to quantify and control by more direct means;

2. to account for species interactions which are not satisfactorily taken into account in single species stock assessments; and

3. to allow recovery of the total biomass from the reduced level in the 1960s to a level giving the maximal or some optimal yield in a fairly short period of time.

Discussions on its use and structure continued during 1974-76, the period of time it was employed in the fisheries of Subareas 5 and 6. However, at about the same time, discussions were also being pursued on other ways of limiting effort, particularly the merits of $F_{0.1}$ in comparison to F_{max}. Consideration of $F_{0.1}$ would reduce the necessity for the relatively conservative two-tier approach. These discussions were cut short by the extension of jurisdiction to 200 miles by both Canada and the USA in 1977, which ended ICNAF's management authority.

Subsequent to ICNAF both Canada and the USA dropped the two-tier approach and used stock-specific TACs as a management tool. Canada continued to do so up to the present time. The USA on the other hand switched over to a more indirect effort regulatory system (closed area/seasons, mesh sizes, minimum landed sizes) in

1982.

Thus the 1974-76 experiment with a two-tier system was unique and can offer some guidance for alternatives to the present system.

The Methodology of the Two-Tier System

The system consisted of the following elements:

A. For each stock, an estimate of the yield for the upcoming year was made, using routine assessment methodology i.e. Sequential Population Analyses (SPA), catch curves, etc., to determine current population size along with F_{max} as the target exploitation rate.

B. The above estimates of yield formed the basis of the TAC regulation. The TACs were then allocated, through negotiation and primarily based on historical performance, to member countries. Thus each country would receive a set of species allocations. This represented the first tier of the system.

C. The next step was to determine the total yield of the system, in this case the Maximum Sustainable Yield (MSY) of all species in Subareas 5 and 6. This was based primarily, if not solely on surplus production models, such as that of Schaefer (1954). This model also indicated current resource status and from this could be determined yields which would allow attainment of the MSY.

D. Based on points B and C, the level of the second tier was negotiated by the Commission and allocated to member countries.

Generally, the allocation of the 2nd tier TAC among the countries was done in such a manner as to minimize by-catch problems i.e. technological interaction, while consideration of an overall MSY was, in principle, to take into account biological interactions. Consequently, ICNAF conducted a considerable amount of research on these two aspects of multispecies interactions.

The Allocation Process

Although the Commission did not officially use any one method for the purposes of allocating the second-tier TAC among the member countries, one technique was used frequently by the USA to support their proposals. This was a linear programming technique using the single species allocations and by-catch ratios to calculate compatability of these allocations. It had the appealing property of giving the lowest overall allocation to those with the most mixed fisheries -- in other words, penalized those with high by-catch ratios.

The procedure was introduced by Brown et al. (1973b; 1979). The problem was to determine:

$$X = (X_1, X_2 \ldots X_n); \text{ such that} \qquad (1)$$

$$Z = \sum_{i=1}^{n} C_i X_i$$

is maximized, where for each i, C_i is the weighing coefficient of the variable X_i. As well:

X_i = catch of species i to be taken in directed fisheries for species i;

C_i = catch of species i in all fisheries divided by catch of species i taken in directed fisheries for species i ($C_i \geq 1.00$);

n = number of directed fisheries considered;

Z = total catch of all species.

Solution, via the Simplex Method, of equation (1) is constrained for each i by:

$$\sum_{i=1}^{n} a_{ij} X_i \leq b_j \qquad (2)$$

$$X_i \geq 0 \qquad (3)$$

where: a_{ij} = catch of species j taken in directed fishery for species i divided by catch of species i in directed fishery for species i. These are the observed by-catch ratios. Brown et al. (1979) used those for 1971 and 1973.

b_j = constraint on total catch of species j for j = 1....m.

The analysis was run simultaneously for all countries. The linear constraint was that no country would exceed its national allocation for any species, b_j. The final product was a vector X for directed catches of the species along with the resultant total catches of the species and the overall total catch.

It is worthwhile to note that the solution derived by Brown et al. (1973b; 1979) provided an aggregate species catch 20-25% lower than the sumed species-specific TACs derived from the stock assessments.

The Determination of the Maximum Sustainable Yield (MSY)

As stated above, determination of total system production was to account for biological interactions. Fukuda (1973) was one of the first to point out that, in multispecies systems, the simple addition of the MSYs for the individual populations in an area was only valid if biological interaction was low. A number of studies were conducted using the Schaefer Model which demonstrated this (Pope, 1975; Pope and Harris, 1975; Pope, 1976a). Pope (1976b) showed that the effect of stock interactions was to

reduce the total MSY of the system below the sum of the individual MSYs. Generally, the greater the interaction, the greater the reduction. Horwood (1976), however, showed that, depending on the relative sizes of the interacting populations, one could obtain non-Schaefer population dynamics. This led to more recent criticizisms of the Schaefer model (Roff and Fairbairn, 1980; Hilborn, 1979; Mohn, 1980; Uhler, 1979).

However, during 1974-76, the Schaefer Model and other total yield models were used to determine the total MSY of the Subareas 5 and 6 resources.

The first estimates were made by Grosslein (1972) who simply added all the individual stock estimates together. Brown et al. (1973a; 1976) updated Grosslein's analysis and using the Schaefer model provided an estimate of 800-900,000 t for total finfish in Subareas 5 and 6. The individual assessment MSYs added to 1,352,000 t, thus, one could argue, showing the degree of biological interaction in the system.

The Establishment of the Second Tier

The TACs that were set during 1974-76 are given in Table 1. The species TACs were derived from assessments and surveys. The Schaefer model and the linear programming analysis were both used to define by how much effort would have to be limited. The precise overall TAC values were agreed to through negotiation. It is interesting to note that the overall estimate was always set 20-25% lower than the summed TAC. This is consistent with the results of the linear programming exercise.

Application of Two-Tier System to Scotian Shelf Fisheries

The tow-tier system was imposed to reduce effort below that necessary to harvest MSY from a given area. When the principle of $F_{0.1}$ was introduced, questions were raised as to the utility of the two-tier system. It had no unique merit as a regulatory tool for fishing effort. However, it was a system that could be agreed to in an international forum, in which results had to sometimes be achieved by rather indirect methods.

The situation today on the Scotian Shelf is quite different. The coastal state, Canada, can impose direct limitations on fishing effort in both the foreign and domestic fleets. For instance, foreign fisheries are allowed (licensed) for only a few species under a system of direct effort and by-catch controls. Observers are placed aboard many of the vessels and fishing closures and/or redirection are considered when by-catch problems occur.

Interestingly, it is within the domestic fishery that most of the management problems occur. Indeed the situation is very much like that present in ICNAF during 1974-76, with countries are replaced by fleets. There are licensing regulations and a detailed system of stock catch allocations which are established through a negotiation process involving the competing fleet sectors. Severe by-catch problems exist and in some areas there is widespread disregard for the quota regulations which are difficult and expensive to enforce.

Application of the two-tier system to the domestic fleet may have merit. One could calculate a second tier for the Scotian Shelf groundfish resources, which would probably be 10-20% lower than that of the summed individual TACs. This would be allocated among the various fleet sectors according to the linear programming approach used in ICNAF. Not only would by-catch problems be reduced, or at least problem areas identified, but also by reducing inconsistencies in allocations, smoother operation of management plans should occur. Enforcement would be eased because once the second tier is reached within a fleet sector, fishing must cease altogether. The major problem with this approach would be industry's perception of and perhaps non-compliance with the second tier. However at the very least, the linear programming could greatly assist in the establishment of fleet specific allocations, a procedure readily adaptable to the inclusion of economic parameters.

In summary, the two-tier system of ICNAF can provide us with a mechanism to assist in present day allocational issues but may not be as useful in curtailing fishing effort through application of an overall TAC.

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Discussion Period

Brodie: We tried a similar linear programming approach to groundfish fisheries on the Grand Banks with much the same results. We found that the second tier TAC was actually about 70-80% of the sum of the individual TACs.

Sinclair: To follow that up, Brown et al. (1973b, 1979) used the ICNAF Bulletin data in which there has not been any attempt to break out the individual fisheries.

O'Boyle: I wanted to look at the by-catch ratios in 1974-76 to see if the by-catch regulations did in fact have any effect. I found that there were difficulties in sorting the fisheries out. There was a lot of catch reported under mixed-directed. In fact, what Brown et al. did was redefine the fishery as directed on the species which compressed the majority of the catch.

Neilson: In your talk, you referred to an over-capacity situation in the Scotian Shelf fleets, whereas in Allan's talk we heard that there was none in respect to the large offshore trawlers.

Sinclair: The over-capacity is mainly in the less than 65 ft. draggers, particularily in Southwest Nova Scotia.

O'Boyle: No one has actually evaluated what the capacity of the Division 4X fleet is; it is a big job. There is only a general feeling that there is an over-capacity situation in that area.

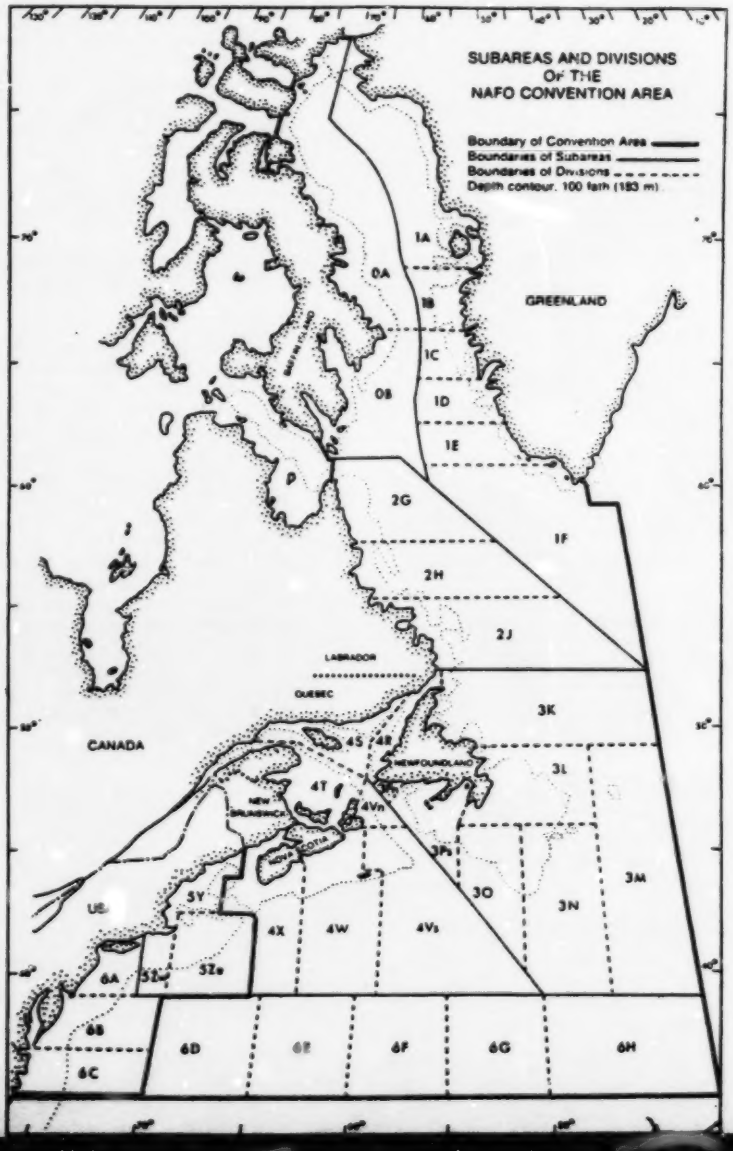
Sinclair: Bob O'Boyle raises a point of concern to me which is that we seem to be thinking of multispecies interactions mainly at the biological level. However, in the fishery as a whole we need to consider the fleet. In Scotia-Fundy I think our problems are more related to the size, structure, location, and capacity of the fleets rather than to biological interaction problems. So in our discussions I think we need to give more consideration to the fleet as the predator.

Mahon: This was definitely the intent when we organized this workshop.

O'Boyle: In regard to this, I found that there has been considerable work done on overall systems, i.e. management of overall catch or overall effort in a particular area. Underlying this view is the idea of man as the predator operating in a density dependent fashion moving from prey to prey. In principal, an overall regulation will work so long as the predator population is in the right area of magnitude i.e. matched to the prey. One management strategy that disrupted the pattern was sector management, which eliminated whole prey sources for many predators. A more realistic approach might be to consider all the fishing communities on the east coast as predators, and the entire array of resources as prey, and start looking at the entire system in that way. We have always concentrated our activities on the prey and have lost sight of what we are really trying to manage, which is the predator, the fleet.

Table 1. Summary of species and overall TACs in place in ICNAF Subarea 5 and Statistical Division 6 during 1974-76.

Species	Stock	1974	1975	1976
Cod	5Y	10	10	8
	5Z	35	35	35
Haddock	5	0	0	6
Redfish	5	30	35	17
Herring	5Y	25	25	7
	5Z+6	150	150	60
Yellowtail	5(E of 69°)	16	16	16
	5(W of 69°)	10	4	4
Other Flounder	5+6	25	25	20
Silver hake	5Y	10	15	10
	5Ze	50	80	50
	5Zw+6	80	80	43
Red hake	5Z(E of 69°)	50	20	26
	5Z(W of 69°)		45	16
Pollock	4VWX+5	55	55	55
Mackerel	5+6	350	285	254
Squid	5+6	65	71	30
Other finfish (and Argentine)	5+6	200	150	150
Sum		1161	1101	807
Overall (finfish and squid)		924	850	650



The Current Approach of ICES to Multispecies Assessment

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In Europe, a group of scientists working in ICES has developed a Multispecies Virtual Population Analysis (MSVPA) which quantifies predation mortality among interacting stocks. A workshop was held in 1984 during which this model was applied to fisheries in the North Sea. Although the results were tentative, the exercise proved worthwhile in better defining the necessary input parameters, the inherent assumptions and their sensitivity, and finally the problems related to evaluating the model's voluminous output. The model cannot yet be used to assist in the management process. It appears innappropriate for use on Canada's east coast due to both the lack of the appropriate types of data and the relative lack of exploitation of age one to three fish in these waters. It is in these age groups that predation mortality is expected to be highest. Canadian fisheries generally exploit older, larger fish and thus MSVPA is less useful. Pursuit of other models more suitable to available data sets and the fishery is recommended.

Introduction

In recent years the International Council for the Exploration of the Sea (ICES) has been actively developing multispecies models for use in fisheries management. Subsequent to the 1984 meeting of the ICES Multispecies Working Group, the Canadian participants produced this synopsis of ICES progress towards development of a multispecies model, along with comments on its potential utility for Canada's east coast. Many of the details of the ICES work were left out. These can be obtained in the working group report (Anon, 1984) which the present document in no way attempts to usurp.

Background

The fisheries in the North Sea generally exploit populations at substantially younger ages than is the case off Canada's east coast. For instance, North Sea (ICES area IV) cod and haddock are fully recruited to fisheries at age three (Table 1) whereas in Canada, values of age five plus are more common. As well, there are large industrial fisheries for Norway pout, sandeel and sprat, all exploiting individuals of age zero to four.

Andersen and Ursin (1977) pointed out that predation can represent a substantial part of the natural mortality of these young fish in the North Sea and elsewhere. In order to obtain a true representation of the recruitment processes in the younger age groups, this must be accounted for. At the 1979 ICES Statutory meeting, Helgason & Gislason and Pope independently presented a method which would calculate these predation mortalities. The method was in essence a Multispecies Virtual Population Analysis (MSVPA). In other words, where the Andersen/Ursin model was a forward looking simulation which made assumptions about recruitment, the MSVPA was a retrospective analysis which used available data to elucidate what happened. It was hoped that examination of the history would lead to more appropriate projection models.

The presentation at the 1979 statutory meeting generated much interest and subsequently an Ad Hoc Working Group was formed to determine the model's data requirements. This resulted in a large sampling program in 1981, generally referred to as the ICES "Year Of The Stomach". The results of this year of sampling were tabled at the 1983 Statutory Meeting.

The Multispecies Working Group was convened in 1984 with the main aim being the application of an MSVPA to the North Sea resources using the 1981 stomach analysis data (Table 2).

Before discussing the results of the meeting, it is worthwhile to review the theory underlying the MSVPA as well as mentioning the input data sources used during this meeting.

The Multispecies Virtual Population Analysis (MSVPA) or Legion Analysis

Whereas in Virtual Population or Cohort Analysis, natural mortality, M, is input, in MS VPA, it is partitioned into M1 (mortality due to disease, starvation, spawning stress, senility, etc.) and M2 (mortality due to predation by other species and to cannibalism). VPAs for each stock included in the analysis are done simultaneously with M1 fixed and M2 calculated according to the size of the appropriate predator populations. Upon completion of the first set of VPAs, new population size conditions will exist and thus the M2 values for each population will have to be recalculated. The analysis is redone with contantly changing M2 values until the convergence criteria as outlined below are met.

The formula used to calculate M2 for each time period is given in equation 1 (Table 3).

M2(y,s,a) is the predation mortality in period y of species s at age a. $D(y,s,a) \cdot W(s,a)$ is the biomass of species s at age a devoured during period y. $\bar{N}(y,s,a) \cdot W(s,a)$ is the mean biomass of species s at age a during period y. $D(y,s,a) \cdot W(s,a)$ is calculated as per equation 2 (Table 3).

Note that in equation 2 the term Food(j,b) is the total consumption of predator j, age b during period y. In the existing model, this is assumed to be constant from period to period. Thus density dependent changes in growth which would affect Food (j,b) are assumed negligible. Predators in this model always get their full ration and only the species mix in the diet changes. This is a major difference from the Andersen-Ursin model which allows for density dependent effects.

The last term in equation 2 is derived in equation 3 (Table 3). SUIT (s,a,j,b) is the availability or suitability of prey s, age a to predator j, age b; it is a positive number between 0 and 1.0 and $SUIT(s,a,j,b) = 1.0$. The indices i and d are the potential prey species and ages used in the analysis.

The term STOC (s,a,j,b) is the fraction of predator (j,b)'s food met from prey (s,a). This is obtained directly from the stomach content data set. Thus estimates of SUIT (s,a,j,b) can be calculated directly (Equation 4, Table 3). As with M2, this calculation uses population estimates, $\bar{N}$ (y,s,a) generated by the MSVPA itself. Thus the calculation of SUIT is an iterative procedure.

Inserting equation 2 into equation 1, gives equation 5, the final calculation of M2.

The MSVPA solves the equations on a time step by time step basis using the following algorithm:

1. Make initial guess at SUIT and M2.
2. Calculate F and N as per ordinary VPA (normally Newton-Raphson iterative solution of catch equation).
3. Based on calculated N values, calculate new M2 using equation 5.
4. If M2 (current) and M2 (former) are not within a certain tolerance, then go back to step 2.
5. Calculate new SUIT as per equation 4, using empirical stomach content data.
6. If SUIT (current) and SUIT (former) are not within a certain tolerance then go back to step 2.
7. END.

Besides the assumption of density-independence, the other major assumption in the present formulation is the nature of the feeding model. This involves the term STOC (s,a,j,b) of equation 2. This term, given mathematically in equation 3, can be phrased as

$$STOC = \frac{\text{Biomass prey available to predator}}{\text{Biomass food available to predator} + \text{Biomass not available (other food) to predator}}$$

Note that in equation 3, the indices i and d in the denominator are used to differentiate the available and unavailable food.

Three models now exist. In the Pope derivation, other food is ignored and STOC is assumed constant. In the Helgason-Gislason derivation, other food is held constant while available food can vary. Finally in the Sparre model, both available and other food can vary but the total (denominator of above equation) is held constant.

Two further items regarding the MSVPA are worth mentioning. First, it is not immediately obvious whether or not the equations are solved uniquely. This is referred to as the Uniqueness problem. Secondly, the stomach content data implicity assumed stomach weights at age for the prey. The formulae use sea weights at age for the prey. For many age classes of some prey, weights at age of prey in the stomach samples differ substantially from weights of prey in the sea samples. This could cause substantial bias.

Both these problems were addressed in the workshop.

Input Data for the Runs at the 1984 Workshop

1. Catch-at-age

The analysis was run on a quarterly basis using catch at age for all available North Sea resources including cod, haddock, whiting, saithe, Norway pout, sprat, sandeel, mackerel and herring. The first task during the meeting was the

compilation and verification of the data from the various assessment working group reports. As a number of species did not have complete quarterly breakdowns, assumptions had to be made. As well, some species catch-at-age included landings from outside the North Sea.

2. Relative Food Compositions (the STOC term of equation 4)

The 1981 stomach data set was used. Haddock had to be excluded as a predator due to data format problems. As well, mackerel and flatfish in cod stomachs were ignored as it was felt that these data were biased by the discarding practices of commercial fishermen.

3. Ration Estimates (the Food term of equation 2)

Predator ration estimates for cod, whiting, saithe and mackerel, taken from the report of the Coordinators of the Stomach Sampling Project, were derived differently for the different species, although all were based on empirical studies. These are described in Section 2.4 of the ICES Report.

Dr. Ursin evaluated these estimates from the point of view of first principles, feeding studies and calculations using the Growth Equation (Andersen and Ursin, 1977) ($dw/dt = Hw^{2/3} - kw$). Generally, the consumptions calculated by the Stomach Group were in fair agreement with the theoretical approach.

4. M1 Levels

Where possible, the working group attempted not to diverge from assumptions made by the Assessment Working Groups, unless species interactions already indicated changes. For large species, the predation mortality was assumed to be low and thus M1 was set to M (M1 + M2) as used in the single species VPAs. For the older age groups of smaller species, which can be expected to suffer some predation, the M1 was initially chosen such that M1 + M2 equals the M of the Assessment working groups. For all species, the M1 was set equivalent for all age groups.

5. The Feeding Model

As discussed above, three models exist (Pope, Sparre and Helgason & Gislason). All three models were used in the 1984 meeting.

6. Fishing Mortalities

Starting fishing mortalities were as per the Assessment Working Group reports.

Results of MSVPA

The Uniqueness Problem

A small subgroup discussed this problem. No new analysis was conducted at the meeting. The group reviewed previous work by Dekker and Magnus & Magnusson. Their comments are given in Section 6.6 of the ICES report.

The group felt that if the 5 conditions stated by Magnus and Magnusson hold, then uniqueness is guaranteed. It even went as far to say that uniqueness may exist when none of these conditions are satisfied. It encouraged further work. Certainly it appeared from the runs that were made, that uniqueness was not a problem.

The Prey Weight Problem

On the first day of the workshop, Daan pointed out a flaw in the formulation presented by Sparre and used in the MSVPA computer model. In essence, the average prey weight-at-age in the sea and in the stomach were assumed equivalent. This may not be the case. For species exploited in the industrial fisheries, average weights at age for the fish in the sea may be more closely approximated using the weights of these species observed in the stomachs of their predators. This is because the industrial fisheries habitually exploit the small members of the population. For groundfish, the situation is even more complex. Dr. Murawski, along with the Canadian participants, conducted an Analysis of Covariance, summarized in Section 2.7 of the ICES report, which examined these relationships. This analysis was part of a larger attempt to come up with a suitable correction factor for the model. Unfortunately the results of this analysis could not be used due to high variability and thus empirical estimates of the stomach/sea weight ratio had to be used. Indeed the feeling was that, instead of devoting effort on the determination of a correction factor, the model itself should be changed. Dr. Sparre suggested the addition of length catagories to the analysis. This would require an entire rewrite of the software. Thus the empirical estimates of the correction factor were used for this meeting.

There was a great deal of discussion on where to apply the correction factor. Two possibilities were entertained. One called for applying it to the nominator and denominator of equation 5, i.e. change the SUIT- values. The other was to run the iterative process and then multiply M2 by the correction factor. As it transpired, only runs using the first option were conducted successfully.

This problem will have to be solved to remove a major source of bias from the current formulation. There were indications given at this meeting that work will be pursued in the near future to address this issue.

MSVPA Run Results

One run, termed the key run, was made to which all other options were compared. It included the Helgason-Gislason feeding model, M1 values as per the Assessment Working Groups and no correction for the stomach/sea prey weight problem.

The results and an example of some program output are given in section 2.8 of the ICES report.

There are few surprises. Fishing mortalities are very similar to the SS (Single Species) VPAs and predation mortalities were very high on age 0 to 1 age groups.

Six additional runs were made, with varying options of feeding model, ration level, M1 and stomach/sea correction factor. Full results are provided in the ICES report. In essence, virtually all runs produced different results. Due to time constraints little discussion on the pros and cons of the various options was possible. This will have to await future meetings.

However one thing became very obvious. Whereas the single species VPA results can be viewed on one page, the results of the MSVPA occupy about 5-10 cm of output. Summary tables are called for. There was some progress made toward this during the meeting, through the generation of predation mortality estimates of each species on each other species. A description of this is given in Section 6.2 of the ICES report.

In addition, Dr. Sparre will investigate the possibility of feeding the results into a structured data base system to allow rapid post-analysis.

Impact on Management Advice Formulation

Short Term TACs

The results of the key run were examined to see whether or not the MSVPA provided any information which would give different short term advice than the single species VPA.

The recruitment estimates from the SSVPA were plotted against those from the MSVPA and except for sandeel very good correlations were observed. Thus, the SSVPA was at least picking up the same recruitment pattern and the MSVPA would not necessarily produce better recruitment/IYFS (International Young Fish Survey) relationships. The high predation mortalities on the young individuals would have a short term effect only if the partial recruitment and/or current level of fishing mortality changed. The Working Group recommended that if a shift is desired, then run 2, which used ration levels 50% of those of the key run, should be used to obtain M2 estimates, even though they were very provisional.

Long-term Yield

It was recognized by all participants that MSVPA may in fact have much more impact on long term, rather than short term yield prediction. However, this raised the question of recruitment variability. Although MSVPA can document the historical interactions and thus allow examination of species interactions, forcasting demands some knowledge of the controls on recruitment, something that still eludes fisheries biologists.

Dr. Shepherd had a program, albeit still under development, with which one can assess the likely changes of yield in all fisheries resulting from a small change in each. The output is small and intelligible. Examples are given in Section 4.3 of the ICES report.

Stomach Sampling in the North Sea

The last major item covered in the Workshop was the adequacy of the stomach information. A first analysis of the variability in the cod data showed high variance which would require more sampling in future projects. Certainly the stratification scheme needed to be closely examined. Also the probability distribution of within stratum data needed to be investigated to allow the use of the appropriate transformations.

Regarding year to year changes in the SUIT matrix, it was felt that a three year intensive stomach sampling program should be conducted for cod and whiting, restricting efforts to a particular season but covering the whole North Sea. This would allow examination of annual changes in diet composition and thus species preference.

Conclusions of Meeting

The activities at the meeting represented the first attempt by ICES to use multispecies calculations in the determination of yield. As such, all results were tentative and more questions were raised than answers provided. It was apparent that MSVPA will have little effect on the short term, as long as current fishing patterns prevail. However, it might arise that industry will want to catch more of the small fish before they get eaten. The long-term consequences could be dramatic. An assessment of these involves some knowledge of recruitment patterns.

Thus the MSVPA has provided a valuable data base on which to conduct analysis of what did occur but does not as yet provide the necessary tools to allow evaluations of long-term impact.

The Canadian East Coast Context

A key difference between the fisheries in Canada and the North Sea is the relatively young age of the fish in the European catches. Thus an MSVPA to determine the predation mortalities of these young age groups makes sense. The same may not be true in Canada.

Each Canadian participant prepared a section describing how the ICES approach could or could not be used in their region. As well, some comment on the longer term utility of the MSVPA is given.

Scotia-Fundy Region

The major problem is availability of adequate stomach data. Data for 1959-69 collected on the Scotian Shelf by Canada are very crude, being qualitative in nature and lacking good prey size information. The quality of the data set improves in 1977 but only becomes suitable for MSVPA in as late as 1982, when work on 4VsW cod was commenced. Later, as part of the South West Nova Scotia Fisheries Ecology Program, good quality stomach data was collected on 4X haddock. On the central Scotian Shelf, silver hake stomach data has been collected since about 1982.

The longest time series of information available was collected on U.S. bottom trawl surveys conducted out of Woods Hole, commencing in 1963. During 1963-72, the information included prey composition and volume but size information was lacking. Since 1973, however, the data set has included size information.

In summary, there exists a variety of data sets, each differing in sampling methods and coverage in time and space. A major effort would be required to pull these rather disjunct data sets together. Even then, only a small fraction of the data may be useful in an ICES style MSVPA.

The workshop pointed out a major problem in the existing model - that is the prey stomach/sea weight problem. Before use of the model this needs to be corrected.

It was mentioned earlier that the stocks in the North Sea recruit to the fisheries at a much younger age than they do on the Canadian East Coast. Here, it can be said that most of the mortality due to predation occurs prior to entry into the fishery. This makes elucidation of the possible multispecies effects all the more difficult. Specific sampling and survey programs would have to be put in place to evaluate pre-recruit mortality processes.

Thus, from a Scotia-Fundy viewpoint, the ICES approach may not as yet be appropriate here. Other ways of attacking the issue which are more suited to the resident data sets and site specific problems need to be investigated to meet immediate demands. This is not to say that the MSVPA should not be used. Rather, it is in the initial stages of development and is not well suited to address regional problems.

Newfoundland Region

A basic conclusion of the ICES workshop was that predation mortalities on young age groups (0's, 1's, and 2's) were often substantially higher than 0.2, whereas predation mortalities on older age groups were nearly 0.0 for most species. Hence, the common use of 0.2 as "natural mortality" for all age groups in a VPA would be substantially in error; underestimating mortality of young age groups, and possibly overestimating mortality of older age groups. The latter point depends on the levels of non-predation related natural mortality, and data on which to base estimates of such mortality are scarce.

These tentative changes in the value for natural mortality will be important for VPA's of North Sea stocks. Because of the industrial fishery and early age at which fish enter the commercial fishery there, VPA's for North Sea stocks usually include data from the 0 age group up. Changing values of natural mortality for early age groups in such VPA's would change the entire analysis, and by a large amount if the change in natural mortality was large. For the Newfoundland Region, few VPA's include data on early age groups. Therefore changing natural mortality values for the 0's, 1's, and 2's would have little effect on the VPA results for older age groups. Initial recruitment estimates would be higher for each year class. Because those values are usually simply the estimated recruitment necessary to produce the required number of 3's (or whatever age group is the first observed value in the numbers at age table) with M equal to 0.2, or estimated from correlations between research survey and commercial estimates of year classes, the consequence is simply replacing one circularly estimated number with another. The actual management component of the VPA would not be changed. Until VPA's for the Newfoundland Region include solid fishery or research survey data for numbers at age of the youngest groups, and until feeding habits of all ages and sizes of all major stocks are known, there is expected to be little additional information gained from repeating the ICES Multispecies VPA methods on Newfoundland stocks.

Quebec Region

Data for application on MSVPA is not available for the Quebec Region. Feeding studies have been conducted on various species but the coverage is insufficient to allow application of the model. The stomach analyses required are probably beyond the capabilities of any single Region on Canada's Atlantic Coast. One reason that ICES was able to have its year of the stomach was that the monetary burden was shared by several countries, each doing one species.

One of the most interesting results from the MSVPA runs was that relative year class sizes were not affected. This would mean that predation does not affect year class size significantly after the 0-groups survey are carried out. It may however be an artifact of using only one year of stomach data. Feeding studies for 4T cod have shown that significant changes can occur in the diet of that species over relatively short periods of time.

As mentioned in the Scotia-Fundy section, a great deal of work remains to be done before the method can be applied on Canada's east coast. It

may not be desirable to attempt to apply it before its merits are clearly demonstrated.

Summary

The ICES Multispecies Working Group has advanced to the stage where it can collate a large amount of multi-stock information in one model in order to provide answers on multispecies interactions. The model is complex, not without its problems and requires further development. In addition it requires a great deal of data. Nevertheless it is expected to provide significant future dividends for the management of the North Sea fisheries once fully developed.

Its utility in Canadian waters is more problematical. First, food habits data on the scale available in Europe does not yet exist here. Second, more importantly, most predatory interaction in Northwest Atlantic waters is suspected to occur in the pre-recruited age groups. Consequently quantification of these interactions through VPA type models will be difficult. Other models need to be developed which both take advantage of the available data sets and also take into account the exploitation patterns in the fisheries.

References

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Andersen, K.P. and E. Urs'n. 1977. A Multispecies Extension to the Beverton and Holt Theory of Fishing, with Accounts of Phosphorus Circulation and Primary Production. Medds. Dann. Fisk.-Havunders. NS. 7.

Discussion Period

Stephenson: What is the strategy as regards the age-related changes in M1 and M2 in this model.

Rice: The idea is that for very young fish the M1 (non-predation mortality) value is small relative to the predation mortality; whereas at older ages and larger sizes the opposite is the case.

Evans: If what is needed is a size rather than age structured VPA, I immediately think of the size structured Georges Bank model. Dr. Murawski, do you see this as being useful.

Murawski: It's a difficult conceptual problem. As far as the utility is concerned we are pursuing Multispecies VPA but not in an all out way. The North Sea is a system where man and fish compete for the same prey. We had a system like that on Georges Bank where man competed with cod for herring but now that herring has declined cod have switched to sand lance, which is not commercially important in North America. So the problem is not so pressing. There has been speculation that silver hake is a controlling species and we want to pursue this, mainly as a framework for sampling. However, as far as the "GEORGE" model is concerned it has been shelved for the time being since when we got into it we discovered that we did not have the data to make it sufficiently predictive for application.

Evans: Could GEORGE be retooled along the lines as MSVPA.

Murawski: I think the fundamental equations are the same; MSVPA and GEORGE are cousins. To comment on the tremendous amount of output from MSVPA, Daan and O'Boyle constructed a two dimensional array which shows the impact of each predator on each prey. This allows back-of-the-envelope calculations of how varying F on one species would influence the population size of another.

O'Boyle: Another point is that of tuning MSVPA. This was not addressed at the meeting. Dave Armstrong looked at the correlations between the recruitment estimates from multispecies and single species VPAs and found them to be very similar. We wondered at that point why we were doing the whole exercise. The point here is that the fishing mortality on some of the younger ages is so high that unless the fishing patterns change the results will not change all that much. However, when industry realizes how much sprat is being eaten by cod, they may change the pattern to get the sprat before the cod do.

McGlade: The way industrial fisheries are reported is predicated on politics of the EEC community. In the Saithe group the scientists agree that the numbers which they bring to the meeting are in order of magnitude too small because the industrial fishery mixes all the young fish together and consequently they are never reported.

Sinclair: I would comment that the predation mortalities on cod are actually less than people were expecting before they did the analysis. At age 0 it was about 0.5 and about 0.2 at age 1 and beyond that essentially nothing. Even for herring which might be considered a prey species, the levels, except for age 3, were about 0.02 or some similar value.

Rice: The herring is particularly deceptive because there were few herring out there. In calculating prey suitabilities, herring were given a low intrinsic suitability because few were recorded as being eaten. They were not being eaten because they were not there to be eaten. Then, when the model is applied to a time when herring were abundant, few were estimated to be eaten because of the low suitability they had been given earlier.

Sinclair: The herring in the Southern North Sea now is said to be about where it was in the 1950s.

Rice: The fish stomachs were collected in 1981 when there were almost no herring out there.

Sinclair: What about the haddock and other species where you might expect interesting interactions? Were the predator mortalities in

the same order as cod? Was there any discussion about how surprisingly low they were?

Rice: The results only came on the last day of the meeting and there was no time for discussion.

Sinclair: I think there was surprise based on the change in Daan's emphasis on how important this analysis was to fishery management: two years ago he thought it was critical, this year his view was that the long term implications are important, but the short term effects on management advice might not be too dramatic. It seems perhaps, and this comes out in some of the other papers, that we may not have very interacting biological systems.

Rice: I am inclined to agree. It is a very interesting analysis but I am not going to say that we should be putting all our resources into a similar one here.

Table 1. Comparison of full recruitment estimates from North Sea and Canadian East Coast.

	NORTH SEA		CANADIAN EAST COAST	
SPECIES	Area	Age	Area	Age
Cod	IV	3	3Ps	7
			3Pn-4Rs	7
			4T-4Vn	6
			4VsW	5
Haddock	IV	3	4VW	6
			4X	5
Pollock	IV	4	4VWX	5

Table 2. Terms of Reference of Multispecies Working Group Meeting (18-22 June 1984)

1. Start trial runs with MSVPA models.
2. Discuss the implication of their results of Multispecies Assessments in the formulation of management advice.
3. Provide advice on possible further needs in relation to collection of stomach content data.

Table 3. Equations used in derivation of multispecies virtual population analysis.

(1) $$M2(y,s,a) = \frac{D(y,s,a) \cdot W(s,a)}{\bar{N}(y,s,a) \cdot W(s,a)}$$

(2) $$D(y,s,a) \cdot W(s,a) = \sum_j \sum_b \text{(consumption of prey s at age a by predator j at age b)}$$

$$= \sum_j \sum_b \bar{N}(y,j,b) \cdot Food(j,b) \cdot \text{(Fraction of food of predator (j,b) obtained from prey (s,a))}$$

$$= \sum_j \sum_b \bar{N}(y,j,b) \cdot Food(j,b) \cdot STOC(s,a,j,b)$$

(3) $$STOC(s,a,j,b) = \text{fraction of food of predator (j,b) obtained from prey (s,a)}$$

$$= \frac{\text{biomass of prey (s,a) available to predator (j,b)}}{\text{total biomass of food available to predator (j,b)}}$$

$$= \frac{\bar{N}(y,s,a) \cdot W(s,a) \cdot SUIT(s,a,j,b)}{\sum_i \sum_d \bar{N}(y,i,d) \cdot W(i,d) \cdot SUIT(i,d,j,b)}$$

(4) $$SUIT(s,a,j,b) = \frac{\frac{STOC(s,a,j,b)}{\bar{N}(y,s,a) \cdot W(s,a)}}{\frac{STOC(i,d,j,b)}{\sum_i \sum_d \bar{N}(y,i,d) \cdot W(i,d)}}$$

(5) $$M2(y,s,a) = \sum_j \sum_b \bar{N}(y,j,b) \cdot Food(j,b) \cdot \frac{\frac{\bar{N}(y,s,a) \cdot W(s,a) \cdot SUIT(s,a,j,b)}{\sum_i \sum_d \bar{N}(y,i,d) \cdot W(i,d) \cdot SUIT(i,d,j,b)}}{\bar{N}(y,s,a) \cdot W(s,a)}$$

$$= \sum_j \sum_b \bar{N}(y,j,b) \cdot Food(j,b) \cdot \frac{SUIT(s,a,j,b)}{\sum_i \sum_d \bar{N}(y,i,d) \cdot W(i,d) \cdot SUIT(i,d,j,b)}$$

The Fishing Industry as a Complex System

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The fishing industry is examined in the context of a complex system incorporating human activities. In order to go beyond descriptive modelling a new paradigm is presented, based on the concepts of "dissipative structures" and the evolution of complex systems. In this view, fisheries are seen as part of a larger system in which 'self-organization' arises from successive in stabilities over varying time scales. In particular, the principle of order through fluctuations is stressed, and the effects of a fluctuation environment as the long term yield of a fishery examined. A simple model is presented in which stochastic fluctuation in the parameters of the logistic equation are shown to have a dramatic effect, for even if fishing activity occurs with an average intensity, which does not jeopardize the fish population according to the mean value of the rate of natural increase, the results demonstrate that extinction will still occur given a sufficiently large variance. Further, if the mean square variance is of the order of half the mean value, then again extinction is shown to occur even though this is supposedly the zone of maximum production.

Background

Fishing is one of the oldest forms of food harvesting. From the earliest records of middens to the highly technological and industrial processing plants, which characterise many existing fisheries, we can see a dependence on fish as a source of food which is unlikely to decline in the near future. Notwithstanding the apparent streamlining of the flow between resource and market, the fishing industry is still affected by uncertainty generated within the marine ecosystem, as a result of the management regimes in place, and of the global pressures for exploitation of natural resources. All of these factors combined, have resulted in the rapid and unprecedented transitions observed in the majority of our ecosystems (Bennett 1976).

In terms of the fishing industry, transitions in the marine ecosystem have been observed, monitored and responded to, in varying degrees. Thus to understand the impact of such changes, we must attempt to identify the processes and causality which serve to interrelate fish and their abundance, fish harvesters' fishery scientists and fishery managers, for all play an important role in the final yield of fish for human consumption. In a schematic diagram (Figure 1), we have identified areas that are clearly of interest in answering some of the more obvious questions that relate to the complex interactions alluded to above. In the marine ecosystem, as it exists today, each one of these 'pigeon-holes' can be said to be affected by all the others, insofar as each species of commercial importance is a member not only of that species but also of a larger fish community or assemblage. Interspecific relationships must therefore be considered normal rather than exceptional, and it is their scale that must be determined. In addition, each species of fish exists in a different way for the assemblage of fish harvesters, some being more important than others in certain sectors of the fishery. In this context, the single-species deterministic models, which have been used by scientists and managers, to describe the interactions between fish and fish harvesters are clearly not circumspect.

In the first instance we must recognize the complexity of the system in which we are involved as fishery scientists. Once we have accepted this, it is incumbent upon us to learn the lessons of those who have gone before us. For example, one of the traps awaiting the user of a systems-theoretical approach, is that it is based on the assumption that real phenomena can be described by a model whose invariant aspects are captured in fixed mathematical relations. Whereas in the marine ecosystem, there is hierarchical interdependence of goals and sub-systems, which cannot be dealt with as simplistically (Mesarovic 1968). But on a practical level, the espousal of an 'holistic' approach, with its attendant optimism and range of vapid generalizations, is unlikely to yield an understanding of the emergent properties of a complex system unless some form of theoretical framework is initially agreed upon. In this sense, it is critical that concepts and developments outside biology be examined, not so that biology may be "reduced" (sensu Nagel (1952)) to physics or chemistry for example, but so that extensions pertaining to complexity may be made.

Thus true analogies must be sought, for as yet we do not have any real intuition as to the way in which complex systems may behave when perturbed or left alone.

The Evolution of Complex Systems

Recent advances in our understanding of the evolution of complex systems (Nicolis and Prigogine 1977; Allen 1982) have shown us that it can be represented by the motion of the system through a bifurcating tree of possible structures and functional organizations. The interactions which exist between the elements of the system (populations of molecules, of cells, of organisms, of human actors, etc.) at a given time can lead to apparently different large scale organizations and structures corresponding to the various branches of the tree. Evolution has therefore two basic modes: the movement along a particular branch representing a purely quantitative change in a qualitatively fixed structure; and the jumping or switching of the system to another branch precipitating a reorganization of structure, triggered either by small chance effects or fluctuations within the system or by external intervention. At such moments, each part of the system, each subsystem, will experience a change in its environment and will therefore be led to participate in the global process of change.

Natural systems are the result of past events, which have occurred at different times, and to different effect in each part of the system - a vast patchwork of loosely coupled histories which have built complex systems with great global resilience and diversity. They are not perhaps 'optimal' in any way as regards 'energy flux' or biomass or any other obvious factor. In any locality the subsystem exists, for the time being, because it is stable in its present environment. Its 'explanation' is historical. If its environment should change or fluctuate, then it may well be driven to extinction.

In fact, the further that a subsystem evolves along a branch, the more it develops its internal structural organization towards one of great efficiency with very specialized parts, dedicated to the particular environmental characteristics of that branch. It becomes more 'efficient' closer to the 'optimal' perhaps, as it retains mutations and innovations which are 'advantageous' in these circumstances. However, when the circumstances change leading the sub-system to switch to some other branch of the evolutionary tree of the larger system, this same 'dedication' and 'specialization' may well lead to its demise and thus to the extinction of the organisms that inhabit it.

But, in natural systems such a catastrophe will occur only for a particular zone found incapable of a sufficiently variable adaptive response to be able to cope with the change in conditions. This is because in natural systems, different locations are only weakly coupled, and selection, coordination and 'information exchange' are local not global phenomena. As Simon (1962) points out in his essay on the architecture of complexity, complex systems can show remarkable survival properties, because the sub-assemblies are tightly connected to themselves, but only loosely to others, and as such, self-recovery can occur through sub-assembly substitutions.

This means that 'evolutionary experiments' are being conducted in a myriad of localities in a relatively uncoordinated fashion. Extinctions, as a result of overspecialization, will only be 'local' events therefore, and will lead simply to the 'liberation' of the space for 're-use' by other organisms. So, what we observe in ecosystems are organisms which are not only surviving in their particular environment, but which have also demonstrated enough potential adaptiveness to have survived the successive 'shocks' of their history.

Any particular system, of course, may be one which is going to become extinct when future changes occur, for which it has no response. But most existing ecosystems will be those which have retained a rich choice of potential behaviour, perhaps at the expense of absolute functional efficiency. This lack of optimality may well be related to the 'energy' and 'effort' expended in allowing the continued presence of 'spare genes', or diverse behaviours which have ensured its survival in the past, and whose continued existence may well ensure the survival of the system in the future.

Complex Systems Incorporating Humankind

When we turn to systems inhabited by humans, we find that the nature of the evolutionary process has changed dramatically. Two critical factors can be recognized that have led to this change: firstly, the invention of means of transportation and communication have introduced strong couplings between the hitherto only loosely connected zones and regions of the globe. Information, goods, people, plants, animals and even microbes can traverse vast distances in little time and for small cost. In this way the selection process has been coordinated over vast areas, and the encounters and information exchanges between peoples have led to diminishing cultural differences. This stronger coupling of all the different regions of the world, means that we now have a single evolutionary 'experiment' instead of the myriads of different ones arising in each locality. This in turn has meant that overall diversity has been greatly reduced, and that a future failure to cope with changing circumstances would lead not just to local but to total catastrophe.

A second factor leading to a change in the evolutionary process is that of information. Scientific progress has led to a situation where the state of a system can be 'known' very precisely and immediately, and this has meant that decisions based on the going rationale, (for example short term profit) have been based on this type of information. Thus again behavioural diversity has been reduced since 'rationality' itself has assumed a global scale. On the other hand, ignorance allowed a variety of behaviours even though the same rationalities and values were guiding the actors. So information has trapped

and channelled responses to an important degree. We must therefore underline the fact that scientific progress, while revealing what a complex system is doing, has failed completely to uncover why or even how it is doing it. Thus, we have been led to a wildly unstable global system reacting rapidly to its own condition, but without knowledge or understanding of where it should be going, or how to get there.

In view of this it has become vital to reflect carefully about our decisions, and to attempt to understand their implications as far as possible before implementing them. The global learning process must change in nature, because the trial and error method of biological evolution and of our earlier socio-cultural development will no longer suffice. It has become too dangerous. Instead we must develop a new view of the evolutionary process based on understanding and knowledge, which will enable us to make choices according to our value system.

Clearly the value system itself must also change in the light of such knowledge, as it must address the question of choice between realistic solutions which can be attained by the system. One important feature of complex systems is that they can only offer a variety of compromises - each with subjectively judged 'good' and 'bad' features. This is mainly because the interactions between the different factors and locations of the system will inevitably create pattern and structure, which will be perceived by the different parts of the system as being advantagous and disadvantagous of a given solution. Knowledge, even if we have it, will thus not avoid the necessity of making difficult decisions.

Understanding the Behaviour of Complex Systems

To develop a new evolutionary strategy, or process of global learning, we must first understanding the behaviour of complex systems. Scientific progress, largely the cause of our present predicament, has been extremely powerful in solving only certain problems. For example the laws of classical mechanics describe and predict very well the motion of a few interacting particles, and equilibrium thermodynamics and statistical mechanics do indeed succeed in predicting the behaviour of large numbers of interacting particles at equilibrium. This knowledge has given us tremendous power to act and, as we have discussed, to span the globe with transportation and communication networks creating huge flows of materials and energy, and to build modern economic systems imposing enormous shocks on the environment. But science has offered little if any knowledge about the long term effects of such interventions.

However the concepts of self-organizing systems emerging from the discovery of dissipative structures, offer us not only a new basis upon which to make precise quantitative models applicable to specific problems, but also a valuable tool for reflection on the general issues involved in managing the evolution of a complex system. We shall address the application of these ideas to the specific problem of fisheries managment, but, before doing so several quite general remarks can be made. Firstly, in human systems, we see that planning and management are concerned with making rational estimates of how, if possible, to improve or maintain the functioning of some part of a larger system. Thus in general they rely on improved technology (doing the same job faster and cheaper), smoothing out bottlenecks and frictions, designating a structural organization in space and time which seems to best fit the prevailing conditions, and thus moving the particular subsystem under management along a path of greater dedication and specificity. This greater efficiency appears to then lead to a reinforcement of the rationality and cultural values involved, through familiarity and economies of scale, which in turn lead to still greater specificity, and a narrowing of natural diversity and increasing fragility or vulnerability of the subsystem should its environment change.

Indeed our very vocabulary reflects this as we distinguish between objects and organisms that fit our particular requirements, and those that do not. We speak of 'weeds', of 'waste products', of 'side-effects', of 'pests', of 'drop-outs', of 'parasites', etc. In general, rational management and planning will always seek to push back the part of the ecosystem populated by such entities, thereby increasing the share left for appropriate objects, organisms or species. To wit, the whole problem of putting words to parts and processes in complex systems tends to distort our understanding, since the real relationships between objects are probably far more subtle than the words 'predator', 'prey', 'parasite', 'leader', 'worker', etc. would suggest, but inevitably we are forced to use these analogies in our efforts to understand.

Using our conceptual model of an evolutionary tree of possible structures, we see then that such processes of specialization and improved efficiency correspond to our modification of the tree to make it much harder for each sub-system to switch and to survive on another branch. Investment in functional infrastructure becomes greater and greater, in which case the adaptive responses of which the subsystem is capable narrow down until it can only perform the functions it is currently occupied in doing, and any change in the environment will lead to collapse.

Currently, the most common response to this, is to move outwards one step and attempt to control or manage the environment of the subsystem. However this inevitably leads to a cross-linking between subsystems and hence to an increase in the complexity of the system to be managed.

The alternative to 'controlling the world' is to understand the mechanisms underlying the evolutionary process, such that external variability can be forseen and adaptive responses retained. Historically, this approach was not adopted because Western man's perception of his relationship with Nature was, and is still to a great extent, one of domination and of manipulation. To achieve this end, information

about the system was required and was provided by a burgeoning scientific community which progressed from early individualistic efforts of amateur scientists to the vast armies of today, equipped with computers and technology capable of producing excessive information about a system -- excessive because of the paucity of theoretical concepts and obvious models to allow us to discriminate between diagnostic and non-diagnostic statistics -- to say what is significant.

Economies of Scale:Time-Related Sequences in Fisheries

The marine ecosystem is clearly complex, for within the oceans there exist both temporal and spatial fluctuations in the physical, chemical and biological domains. Physical mechanisms, such as mixing, tidal forcing and the presence of planetary waves, have periodicities ranging from hours to decades, across hundreds of kilometres (Denman and Platt 1978; Horne and Platt 1984), whilst some chemical events, such as oxygen uptake and hydrostatic bonding can occur within nano-seconds across areas of less than a millimetre. Despite this, however, biological rhythms, whether diel, annual, inter-annual or decadal, continue virtually unabated: it is only in the aftermath of events such as the grounding of the Torrey Canyon, El Nino or excessive exploitation of a single species, that the seeming persistence of biology is upset.

Links between the physical, chemical, and biological elements of the marine ecosystem are clearly present, as evidenced in such works as those of Bernal (1979) on zooplankton fluctuations, Fournier et al. (1977) on phytoplankton productivity and Iles and Sinclair (1982) on spawning stock sizes of herring (Clupea harengus), but what is perhaps just as important in a fisheries context is the relationship between the spatial aggregation of fishes versus their temporal exploitation. Fish are generally caught where they are concentrated to spawn, to feed, or to migrate from one area to another. For example, Uda (1959) documents the fact that bluefin tuna (Thunnus thynnus) are caught along the western boundary of the Kuroshio during January and March, where large concentrations of sardine-like fishes occur, and on the Scotian Shelf, the spatial distribution of the pollock (Pollachius virens) fishery during the period of November to February, reflects pre-spawning and spawning aggregations (McGlade 1983). Thus, to truly appreciate the impact of fishing, we must understand the distributional features of both fish abundance and fishing effort, plus the various time scales associated with seasonal fluctuations and long-term cycles. We can view the latter cycles as providing us with some form of yield potential, which overrides the idiosyncracies of the various aspects of population dynamics. Moreover they may also reflect some form of predator-prey oscillation which could follow a type of Lotka-Volterra relationship with a variable delay function equivalent to the learning ability of the fish harvesters.

Effects of a Fluctuating Environment

If there exists a long-term yield for a particular species in a particular area, then we must clearly re-examine such issues as environmental and recruitment fluctuations. In a paper, examining the effect of random fluctuations in production, Doubleday (1976) concluded that removal of the Maximum Sustainable Yield (MSY) each year would lead to disaster, although this statement was conditioned by the applicability of a Schaefer model (Schaefer 1954). Nevertheless, recruitment in groundfish stocks does appear in the main to be a stochastic process, except perhaps in times of low spawning population abundance. With this in mind, we have therefore examined the potential impact of a fluctuating environment on such a system as that of a fishery.

As has been noted in earlier articles there are at least three types of fluctuation which must be considered when trying to understand the nature, the structure and the future of any particular system under study: a) the internal fluctuations of variables of the dynamics, b) the external fluctuations of parameters characterizing the environment, and c) the occurrence of new types and the new behaviour of mutants. Now, considering the first type, a), already introduces the view of evolution as a 'dialogue' between macroscopic determinism and simplicity, and microscopic stochasticity and detail. It has been considered in a whole series of papers and books, and broadly speaking shows us how complex systems evolve through successive structural instabilities, a morphogenetic, non-conservative evolution of structure and form. Similarly, some studies have been made of the effects of the third type of fluctuation, c), and it has been shown that new insights into the evolution of ecosystems can be gained by studying the successive modifications that can occur in trophic structure as a result of invasions of randomly varying types of new individual. This has been treated both in its deterministic average form and in its stochastic, more correct formulation. Once again it turns out that taking into account the effects of fluctuations and stochasticty has a major effect on our understanding of the evolutionary process. Instead of a rigid Neo-Darwinism, where there is a clear separation between a lethal and an advantageous mutation with the system evolving towards some maximal efficiency, we find a softening of the selection process (Allen and Ebeling 1983), and strong possibilities for the system to explore options far from the most efficient forms at a given time. It is this that gives systems the capacity to arrive at radical new functions and levels, and to a dramatic restructuring over time. This stochastic softening of the cutting edge of Darwinian selection in fact allows the system to probe and 'imagine' quite different ways of doing and of being, and it is this that constitutes the adaptive potential of the system.

An important remark here is that although the actual dynamic evolution of a system may require a study of the effects of internal fluctuations of type a) and c), the stationary states are nevertheless given correctly by the simple deterministic approach.

However, when we turn to consider the effects of the second type of fluctuation, b) this is no longer the case. As we shall see the macroscopic stationary states observed for a system will be affected by the fluctuations of environmental parameters around their mean values. Even if the variations are extremely rapid compared to the 'dynamics' of the system, the stationary states observed for non-linear systems will not be those corresponding to the solution of the deterministic equations using the mean value of the parameters. Thus the extrema of the distributions of probability will not coincide with the predictions obtained from the non-fluctuating model.

In this way, when we observe real systems in their natural environment, and we attempt to model them using deterministic models with the average values for given parameters, it is entirely possible that the calibration of the model is quite false, due to the fact that the state observed, and its variations over time, may correspond to a non-linear system reacting to its fluctuating environment. This should be born in mind when discussing fisheries models, for example, where future yields are inferred from some set of equations.

An important aspect of parametric fluctuations is that in general their effect is not scaled by the size of the system. In the case of type a) on the contrary, the relative importance of internal fluctuations is scaled by some inverse system volume factor, but this will not be the case necessarily for the fluctuations of kinetic parameters. For real systems plunged into a real environment there will be either some mean value of a parameter, , with variation around this, or some systematic or cyclic trend in the parameter and an apparantly random motion around this. In order to speak of noise, however, we must attempt to establish the existence of a clear cut separation between the macroscopic dynamics of the system, and the rapidity of the variations of the parameters involved.

Turning more explicitly to the problems of modelling a fishery and the impact of environmental fluctuations on the development of a management strategy, let us break the model into two parts and consider the population dynamics of a fish species, under the specified activity of fishermen. In general, the population of any single species will change because of its interaction with components of the marine environment (Figure 1). The model must therefore contain mechanisms reflecting the time scales of the natural population dynamics (age to maturity, etc.), as well as those arising from the changes occurring in all the other species which are interacting with it, plus those of the parameters characterizing the marine environment itself. As a first approximation we can study the effect of supposing that all these different fluctuations act on the population dynamics of a particular species as a white noise perturbation of the rate of natural increase and mortality, and also perhaps, on the rate of fishing mortality. Recent work in the domain (Horsthemke and Lefever 1984) permits us to study the effects if the noise is not really 'white' but has some finite correlation time in it, in which case it is referred to as 'coloured' noise. However, as we shall see, the important point of principle is already posed by the imposition of white noise perturbations on the parameters which we have mentioned.

The second part to the whole model which interacts with the 'fish sub-model' is that describing the fishing activity. Again several time scales will be apparent such as those characterizing total fleet sizes, and the fractions fishing particular zones. The total size may reflect both the business cycles and the relative success of the industry over recent years. There is also a short term set of mechanisms relating to local weather conditions, fishing regulations and the spatial choices made by the different fisherman. However a model capable of describing these phenomena is being developed (pers. comm. P. Allen and J. McGlade), and so we shall not discuss this part of the model any further.

Simple Model of Exploited Species with Fluctuating Environment

Let us return to the question of modelling the effects of the fluctuation of the parameters in the equation governing the population dynamics of a particular species in a given zone. In order to illustrate the effects on the system, let us first consider a simple logistic equation governing the growth of a fish population to some maximum level in a given zone.

$$\frac{dx}{dt} = bx\left(1-\frac{x}{N}\right) - mx - syx; \qquad (1)$$

$$x^0 = N\left(1 - m + \frac{sy}{b}\right)$$

Where x = population density, b = birth rate, m = natural mortality, s = cross-section between x and y; y = number of boats, and N = a limiting factor. This problem was treated originally by Beddington and May (1977) but since then, the theory of stochastic differential equations has advanced sufficiently so that the analytical solution to this simple equation and to other more realistic ones can now be given (see Horsethemke and Lefever 1984). Here we shall summarize very briefly some of these results but the interested reader should study the book.

Firstly, the importance of these white noise fluctuations depends on the fact that they occur in non-linear differential equations, not in a simple additive fashion, but in the value of a parameter which may control a process involving some of the variables. Thus, in our equation (1) we may suppose that the rate of net increase, b-m-sy will in reality fluctuate around a mean value with some variance. This can be written as

$$dx = \left[\overline{(b-m-sy)}\ x - \frac{bx^2}{N}\right] dt + \sigma x dW_t \qquad (2)$$

Now this stochastic equation can be

represented by an equation governing the probability of finding a particular value of x, p(x,t), at time t. The evolution of this probability distribution is governed by a Fokker-Planck equation, and this can in general be solved in the stationary state. The solution can be written, furthermore, in a form which allows us to imagine that the behaviour of any particular system is governed by a potential function, although, in reality it is simply governed by the distribution of probability.

$$dX_t = f(X_t)\,dt + \sigma g(X_t)\,dw_t \qquad (3)$$

$$\frac{\partial P(x_1 t)}{\partial t} = -\frac{\partial}{\partial x} f(x)\,p(x_1 t) + \frac{\sigma^2}{2}\frac{\partial^2}{\partial x^2} g^2(x)\,p(x_1 t)$$

$$(x_1 t) = Ne^{\left\{\frac{-2v(x)}{\sigma}\right\}} \text{ where } v(x) = \text{'pseudo potential'}$$

$$v(x) = -\left[\int^x \frac{f(u)}{g^2(u)}\,du - \sigma^2 \ln g(x)\right]$$

Now, this quite general solution can be applied to the problem posed by equation (2). We can define the functions f(x) and g(x) and hence calculate the stationary probability distribution. It is:

$$f(x) = (\overline{b-m-sy})x - \frac{b}{N}x^2 \qquad g(x) = x$$

$$P_s(x) = \Omega e^{-\frac{b}{N}\frac{x}{\sigma^2}} x^{2\frac{(\overline{b-m-sy}-1)}{\sigma^2}} \qquad (4)$$

(normalization)

In fact this is integrable from 0 to α only if,

$$\overline{b-m-sy} > \sigma^2/2$$

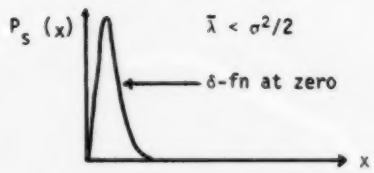
$$\text{let } \bar{\lambda} = \overline{b-sy-m} \qquad (5)$$

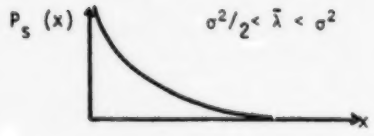
and so studying this we can see that there are two transitions, which may occur in the form of the distribution, as the mean values and the variance are modified.

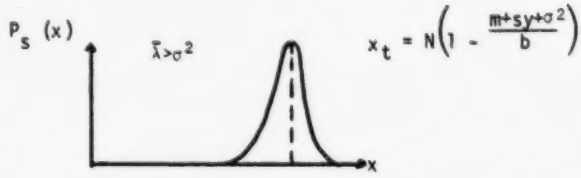
When $\bar{\lambda} < \frac{\sigma^2}{2}$ then, the distribution is a δ - function at zero. We have extinction.

When $\bar{\lambda} < \sigma^2$ then, the distribution peaks at zero, but has a roughly exponential tail extending to positive x. This means that the most probable value of x observed will be near 0, but that from time to time there will be 'bursts' of population. The 'mean' value of the distribution will not be zero.

When $\bar{\lambda} > \sigma^2$ then, the distribution becomes a 'bell-shaped' function situated with its extremum at the value of the solution of the macroscopic equation. The mean and the mode roughly coincide.







The importance of this result is that even if fishing activity occurs with an average intensity, which does not jeopardize the fish population according to the mean value of the rate of natural increase, then extinction will still occur if the variance around these mean values is sufficient. It also means that if the variance is not a matter of choice, but is 'fixed' by the natural environment, then as the fishing rate, or effort, is increased, so the value of b-m-sy drops and the system approaches the danger zone of the transition to zero, even though b-m-sy itself may be quite large.

This type of result was presaged certainly by the work of Beddington and May (1977), and by others since, but now these general methods of solution exist it is possible to extend the analysis to more realistic models. The point of principle is that the fluctuations cause not only the variables to change, but also the potential landscape in which they move, and it is because of this that new phenomena can occur.

Pushing this small example a little further, let us consider the idea of attempting to fish at some maximum sustainable level on the basis of (1). The maximum catch corresponds to a value of x such that,

$$\text{max. catch} = \max(syx) = \max bx(1-x/N) - mx ;$$

$$x^{\text{max. catch}} = N/2\ (1-m/b)$$

$$sy^{\text{max. catch}} = b(1-x^{\text{max. catch}}/N) - m = \frac{b-m}{2}$$

So, a transition occurs when

$$\overline{b-m-sy} = \sigma^2 \rightarrow \frac{b-m}{2} = \sigma^2$$

Thus, at this level, we see that if the mean square variance is of the order of half the mean value then extinction will occur even though this is supposedly the zone of maximum production. A glance at the experimental results relating stock size to recruitment show us that there is indeed a highly uncertain relationship, which can, until some better mechanism can be elucidated, be represented by a white noise fluctuation around a mean value for the rate of natural increase. The question that needs to be addressed now concerns the size of the 'variance' that should apply to a fish population, and also to the construction of more realistic equations. Many more complicated equations can and have now been studied, and therefore much of the difficult theoretical work is already done. What remains is to undertake a proper study of appropriate fishery equations, and in particular find the correct parameter ranges.

Such an undertaking should help to outline more realistic management policies for the long term survival of the fishery, and this, coupled with the models of the spatial and economic behaviour of the fishing boats under a given management strategy which will be outlined elsewhere (pers. comm. P. Allen and J. McGlade), should provide a useful preliminary tool for the construction of a long term fishery policy.

Concluding Remarks

Thus in conclusion we are left with some obvious questions unanswered, and which must be addressed in the immediate future.

1. What are the objectives of fisheries management? If these are not strictly biological, then biological measures of the status of stocks will not provide adequate information.

2. To what extent are fisheries managers involved in a stochastic game rather than a deterministic one? If fluctuations in recruitment are stochastic, what is the shortest period in which a collapse could be recognised and fishing stopped?

3. What are the effects of fluctuations in recruitment on fishable biomass and sustainable yield, both temporally and spatially?

4. What are the effects of spatial fluctuations in the behaviour of fishing boats, and delays in learning?

5. Are the long term cycles that we see in fisheries data, the result of environmental perturbations or "predator-prey" oscillations?

6. By setting TACs can we reduce over-capacitization or do we simply exacerbate the short term profit modus operandi?

We must realise that a fishery is just one part of a larger evolving system, and as such we face a basic choice between trying to manage it or simply leaving it to a 'free market'. If we choose to manage it well, then we need to develop models which incorporate not only an understanding of the fishery itself, but also an awareness of the system in which it is embedded.

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represented by an equation governing the probability of finding a particular value of x, p(x,t), at time t. The evolution of this probability distribution is governed by a Fokker-Planck equation, and this can in general be solved in the stationary state. The solution can be written, furthermore, in a form which allows us to imagine that the behaviour of any particular system is governed by a potential function, although, in reality it is simply governed by the distribution of probability.

$$dX_t = f(X_t)\,dt + \sigma g(X_t)\,dw_t \qquad (3)$$

$$\frac{\partial P(x_1 t)}{\partial t} = -\frac{\partial}{\partial x} f(x)\,p(x_1 t) + \frac{\sigma^2}{2}\frac{\partial^2}{\partial x^2} g^2(x)\,p(x_1 t)$$

$$(x_1 t) = Ne^{\left\{\frac{-2v(x)}{\sigma}\right\}} \text{ where } v(x) = \text{'pseudo potential'}$$

$$v(x) = -\left[\int^x \frac{f(u)}{g^2(u)}\,du - \sigma^2 \ln g(x)\right]$$

Now, this quite general solution can be applied to the problem posed by equation (2). We can define the functions f(x) and g(x) and hence calculate the stationary probability distribution. It is:

$$f(x) = (\overline{b-m-sy})x - \frac{b}{N}x^2 \qquad g(x) = x$$

$$P_s(x) = Ne^{-\frac{b}{N}\frac{x}{\sigma^2}x^2\frac{(\overline{b-m-sy}-1)}{\sigma^2}} \qquad (4)$$

(normalization)

In fact this is integrable from 0 to ∞ only if,

$$\overline{b-m-sy} > \sigma^2/2$$

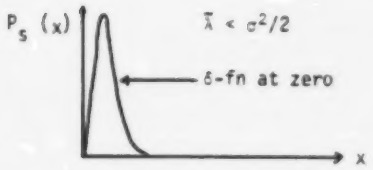
$$\text{let } \bar{\lambda} = \overline{b-sy-m} \qquad (5)$$

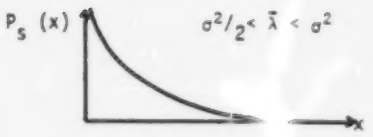
and so studying this we can see that there are two transitions, which may occur in the form of the distribution, as the mean values and the variance are modified.

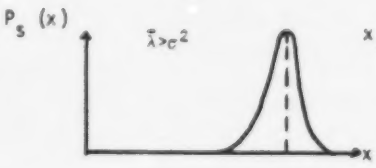
When $\bar{\lambda} < \frac{\sigma^2}{2}$ then, the distribution is a δ - function at zero. We have extinction.

When $\bar{\lambda} < \sigma^2$ then, the distribution peaks at zero, but has a roughly exponential tail extending to positive x. This means that the most probable value of x observed will be near 0, but that from time to time there will be 'bursts' of population. The 'mean' value of the distribution will not be zero.

When $\bar{\lambda} > \sigma^2$ then, the distribution becomes a 'bell-shaped' function situated with its extremum at the value of the solution of the macroscopic equation. The mean and the mode roughly coincide.







The importance of this result is that even if fishing activity occurs with an average intensity, which does not jeopardize the fish population according to the mean value of the rate of natural increase, then extinction will still occur if the variance around these mean values is sufficient. It also means that if the variance is not a matter of choice, but is 'fixed' by the natural environment, then as the fishing rate, or effort, is increased, so the value of b-m-sy drops and the system approaches the danger zone of the transition to zero, even though b-m-sy itself may be quite large.

This type of result was presaged certainly by the work of Beddington and May (1977), and by others since, but now these general methods of solution exist it is possible to extend the analysis to more realistic models. The point of principle is that the fluctuations cause not only the variables to change, but also the potential landscape in which they move, and it is because of this that new phenomena can occur.

Pushing this small example a little further, let us consider the idea of attempting to fish at some maximum sustainable level on the basis of (1). The maximum catch corresponds to a value of x such that,

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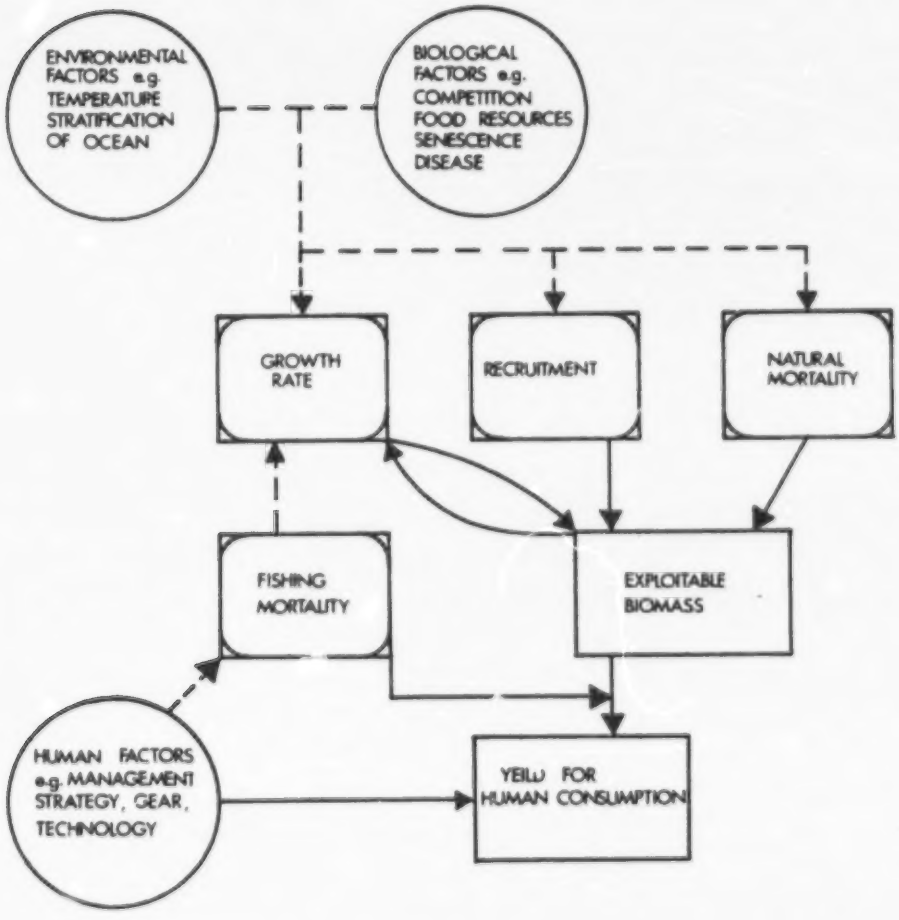

Figure 1. Schematic diagram identifying areas of interest in fisheries.

SESSION V:

GENERAL DISCUSSION AND RECOMMENDATIONS

Preamble

This session falls into three parts. The first is a discussion of past performance, with a diversion into the question of the role of the biologist in management. The second is a consideration of how to incorporate analyses of technological interactions into management advice. The third is how to deal with species interactions. Throughout, there is the consideration of how MEES should function and what topics it should deal with.

Discussion

Chairman: The objective of this session is to formulate some practical recommendations based on the preceeding sessions. To facilitate this, a representative from each session (chairman or rapporteur) and I have summarized the main points which we believe could lead to useful recommendations.

To begin with, we asked how well the current single species models worked. We concluded that whereas we had been successful in rebuilding some, mainly groundfish stocks, others have not recovered thus far e.g. Georges Bank herring and Grand Banks haddock. Furthermore, as we attempt to manage at equilibrium levels we suspect that the models will not be finely tuned enough to produce satisfactory results.

The reasons are 1) that the jury is still out on how well the models have been applied; we may just not have put the right data into them, 2) they may require refinement, a process which will clearly continue; and 3) the models may have failed because they are inappropriate, inasmuch as they do not include the kinds of interactions which we have been discussing over the past two days.

Evans: There appear to be two aspects to the latter point. The first is that the models are inappropriate, and the second that they are single species models. There could be appropriate single species models which have not been tried yet.

Waldron: Are we just going to consider the appropriateness of the model as we use it or are we also going to consider the appropriateness of the way it is ultimately applied.

Chairman: I had intended only to consider the first level for the moment.

McGlade: I think there should be some recognition of how they are sometimes misapplied at the levels of the managers. We know there are caveats which we put on the advice which often seem to disappear in later usage.

Chairman: Assuming that the single species models we use are inappropriate because they fail to take account of interactions of the sort which have been discussed here, we need to come to some conclusions about what we can do. For a start, it seems clear from the workshop that we are in a position to attempt to take into practical consideration the technological interactions, which we know exist and which we know are producing problems in some fisheries. We need to make the point that this will require some reorganization in the way that catches are allocated. We felt that the tools existed to implement this type of approach within a year or two.

Moving on to the area of biological interactions it appears that the best starting point would be to identify small systems of two to three species, i.e. key interactions, and to try to include these explicitly in the management advice. In the paper by Halliday and Pinhorn we heard that this type of information is given some qualitative consideration but that the managers want to see some hard evidence before including this information explicitly in a management plan.

It seems clear that the only place to start is with the small system. However, it also must be made clear that in doing so one could be taking the system out of context and that this could produce some unpredictable consequences. Essentially, it would be a form of experimental management. Any type of multispecies advice has that experimental aspect to it. So, the recommendation is that we move toward explicit inclusion of interactions, at the level of small systems of two or three species.

Going beyond this level of complexity the models quickly become structurally very complex and require committment to data gathering and modelling which are beyond our immediate scope, for example, the ICES multispecies VPA described by Rice. At this stage the only possible way to proceed within the foreseeable future would be to apply whole system approaches which attempt to incorporate overall inputs and outputs, or system responses, into the management structure. However, as has been pointed out earlier, these emergent properties are seldom in terms which can be related to the properties which the managers use to regulate the system. Therefore, unless they can be translated back into some values which relate to single species catches, they will be of limited value.

Finally, in this approach is the assumption that there is a definable system which can be considered as an ecological whole. The size, comlexity, and openness of the ocean makes it unlikely that there are meaningful ecological systems, even if we could define them, which it would be practicable to model for management purposes. Therefore, any attempts to define systems will be at the operational level, based on such practicalities as fleet fishing patterns, monitoring and enforcement capabilities as well as species distributions and environmental regimes. In this connection the session on system identifiation brought to our attention several methods which could be used to identify operational systems. From these, there would be a process of redefinition and application. Again it is necessary to stress the fact that in defining and applying systems in that way one can be ignoring important connections and linkages which could result in unpredictable behaviour of the system.

[Given the above overview the discussants turned to the beginning with the aim of working through the sequence of ideas.]

McKone: Is the objective to increase precision for next years assessment, or to gain a better understanding of the system in the long term, or both?

Chairman: Essentially both, to be realistic our near term objective should be to use the kinds of information on technological and biological interactions we have been discussing to refine the kind of advice we currently provide, while attempting to explore alternatives, for example more consideraton to the other major forces, such as economic and social, influencing the fishery.

[Here, the discussion diverges into a consideraton of the role of the biologist in the management process.]

McKone: I wonder whether the objective of biologists is to give biological advice, and whether they should be concerned with what happens to that advice thereafter.

Chairman: What I gather we might be saying as biologists is that we cannot give adequate biological advice based on biological consideratons alone and that to function effectively as biologists we may need a forum in which to interact with other disciplines.

McKone: I think the question still stands as to whether the biologist need be concerned with more than providing the best biological advice.

Waldron: This is a very important area which we should resolve before continuing our discussion. Although our advice is primarily biological in nature we need to be constantly aware of the effect of our advice on other facets of the management process and vice versa.

McKone: To continue as the devils advocate, I wonder if the biologist can consider the social, economic and political aspects of the fishery and still give the best possible biological advice based on the long term conservation of the resource. Nonetheless, I do agree that biologists could have more input into the management process.

O'Boyle: In my talk I agreed with the last point made. I wanted to try and insulate CAFSAC from the socioeconomic influences. Indeed, my point is that we should provide the best models which are available for fishery production and management and then leave it up to the managers to use them to explore the consequences of various management options.

Sandeman: One aspect of O'Boyle's paper which was lacking was that there is a pathway back from the managers to CAFSAC so that they can put management questions to CAFSAC. However, in reality it turns out that the managers are too busy to spend much time thinking about questions they should be asking, and the biologists have to anticipate them in providing advice. Hopefully we will move towards a situation where there is more interplay, and I think that setting up an equivalent socioeconomic advisory body would contribute to this. However, it is clearly important to keep advice separate from management, while attempting to make it responsive.

McGlade: I think that interaction would definitely be facilitated by having a socio-economic advisory structure parrallel to the biological one such that the two could come together as appropriate.

Sandeman: Yes, this is certainly a serious lack, and indeed they do not have much of a data base to operate from.

A. Sinclair: I would like to comment on the objective of management being to continue to harvest at $F_{0.1}$ through the 1980s. We have found as a group that past levels have been well in excess of $F_{0.1}$ levels. This is due to the willingness of industry to harvest at levels in excess of $F_{0.1}$ and our inability to provide accurate estimates of $F_{0.1}$ yields. Having been unable to achieve this goal in the past, the stated objective of continuing to do so in the 1980s seems naive and unrealistic.

[This comment refers to some retrospective analyses of performance e.g. CAFSAC Res. Doc. 84/81 and 84/100]

McKone: Ultimately, the policy decisions are not in the control of the biologist. These decisions are taken at a higher level and if the decision is made to eliminate the stock by setting catch levels which are too high, that is outside the biologists area of control. The most the biologist can do is advise on the implications of taking certain decisions, and describe the consequences of past management decision.

Stevenson: Is the role of the biologist to do more than adequately describe the dynamics of the populations being managed, and in the multispecies case to understand the interactions which are important? The management implications would apnear to be a separate issue which can be separated out at a very early stage.

Evans: That is true, provided that at the same time the biologist points out as forcefully as possible what the consequences of the management decision are likely to be. There is a difference between saying what should happen and predicting (or estimating, if 'predictng' seems to claim an accuracy we can't deliver) what will happen. We can advise on both, provided we make it utterly clear that we are answering essentially different questions (maybe going to such extremes as using different colours of paper). It is our duty as biologists to say what we think will happen, whether we are asked or not. It is our duty as public servants to say what we think should happen when asked; and we are free as citizens to offer unsolicited advice whenever we think it valuable. As long as everybody knows when we are and are not using the word 'should', there is no problem. To speak of one or other as 'the' objective of biologists is unnecessarily restrictive.

Waldron: Let us look at one instance where we should perhaps be involved. That is in allocation and the by-catch regulation that results. In 4X cod, it appears that the quota would not be caught under the allocation given to the offshore fleet. In the meantime the inshore fleet with a limited allocation and surplus capacity could be engaging in discarding and under the table sales. If we could be involved in advising on the allocation procedure we could say that the TAC is not likely to be taken under the allocation scheme planned. We could at least be more involved in the process of advising on the biological and technological implications of various resource allocation decisions.

McGlade: In fact there is one skill that few of us have any training in; that is the implications of decision making. We can do it in a qualitative way, but do not have the formal skills.

A. Sinclair: Furthermore, we depend so much on fishery information that we have to consider the effect of decisions on that information.

McGlade: Yes, we need to understand the implications of various decisons on what we are doing as biologists and in that way we need to be more involved in the decisions. We cannot really remain isolated from management. Perhaps another framework is required for understanding the implications of decisions.

McKone: We should remember that we do have representation in the decision making organizations -- or is this inadequate for information flow?

[There was an exchange of comments relating to the concern that management is mainly oriented towards dealing with short term crises and reactions to immediate problems.]

Chairman: I would like our attention to proceed on to the second question, which is -- what can we do at the moment about incorporating current information and analyses on technological interactions into the management process? Earlier I concluded that this was an area in which we could hope to make progress in the near future and that we were essentially inviting the managers to ask "how could we optimize some aspect of combined species catches in a particular area?".

Sandeman: I suggest that a working group comprised of the biologists who are experts in this area, and the managers, be formed to investigate the feasibility of implementing some of the findings discussed at this workshop.

O'Boyle: That group could then report to MEES, perhaps at a subsequent general meeting.

[There was agreement on that point and Recommendation 2 was formulated.]

Chairman: With agreement on that point, we should now consider what we could do in regard to species interactions. The point made earlier was that we should begin by attempting to explicitly include information on small systems in the management advice.

Lilly: With regard to the cod-capelin interaction we are probably some distance away from having an applicable model. However, it would be valuable to have a forum in which to evaluate progress in this area, and in which to judge at what stage the results can be usefully applied.

O'Boyle: It appears that we could define a few specific systems, such as cod-capelin and silver hake-haddock as being of potential importance in fishery management. Progress in these areas could then be reviewed by MEES annually until the information was deemed ready for use. Then it could be brought before the appropriate subcommittee in the assessment process. Unless we adopt this operational approach we will have difficulty getting started. The alternative is to define a large system, undertake a broad study of system function and not apply any of it until the job is completed. This I believe has been the approach with the Georges Bank model and the ICES multispecies VPA.

McGlade:To identify important subsystems in which we could hope to make significant progress would be a practical approach in the beginning.

Anderson: Therefore, we are recommending that we regularly review progress in specified areas where it appears that accounting for interactions could contribute to improved advice. However, it is difficult enough to demonstrate that interactions exist, let alone that such interactions are of significance to management.

Evans: Moreover, as we begin to model these small systems we will certainly find that they behave more elaborately than we would have believed possible.

Murawski: This relates to the sort of iterative system definition approach which we discussed previously where we start out with a relatively definable system based on a particular fishery problem. however, for example in the case of the haddock-silver hake problem, it becomes complex very quickly because what we have is a two-stage harvesting system. The foreigners harvest the silver hake and the Canadians harvest the haddock which the silver hake are eating. Then there are other possible interactions with other prey species. Nonetheless, the approch is based on a particular problem. In a whole system approach the first difficutly to be encountered is that massive data collection is required. With the small system/fishery problem oriented approach, this difficulty is substantially reduced and as data are collected one can progressively redefine the system being considered.

O'Boyle: These are fundamentally different ways of approaching the problem. Many modellers would insist that we grasp the functioning of the whole system before attempting to apply any of it.

McGlade: I don't think any of us are naive enough to believe that these systems do function in isolation.

O'Boyle: As a general question -- are there any models of biological interactions which are close to implementation? Were there any in the reviews? [silence] None at all?

McGlade: There is a general need for some kind of review process. I can think of several topics which could benefit form that kind of pre-review. For example, stock structure in relation to the environment.

[A discussion followed on possible ways of coping with the broad array of topics within the mandate of MEES. Some topics were thought to be better dealt with in specialized workshops whereas it was felt that others could comprise a small number focal topics for an annual meeting. Some specific problem areas were:

a) review of working group progress in technological interactions;

b) silver hake-haddock; and

c) cod-capelin.

Another topic which was raised was the influence of environmental factors on recruitment as discussed in the paper by Koslow, and on availability. A previous meeting of MEES had recommended this topic for consideration..]

O'Boyle: A major point has been raised by work presented by Koslow. Basically, it says that the environment is the main signal in recruitment of all our stocks. In the meantime Trites, Loucks and Thompson are working on understanding the physical environmental patterns in the area. When they are finished they will want to tie it into the biology of the area. This could have important mangement implications and should be reviewed in this subcommittee.

Frank: On the basis of what has gone on in the past, one would not get the impression that this is viewed as an important aspect of fishery management. There are a number of potentially important correlations around, for example the work of Sutcliffe, which could have been used to check catch trends. As pointed out by McGlade we often rely on synonomy in making decisions, but here is another type of independent check in confirming a trend which has been noticably overlooked or ignored in preparing management advice.

Sandeman: I think Frank's point is important. Past work has made predictions about catch and we have not looked to see if the models have worked.

O'Boyle: MEES is the ideal forum in which to explore the value of these approaches for managment. Therefore, I would recommend that as the environmental work becomes available we should review it in MEES.

Chairman: To conclude this discussion, we should consider the possibility of using whole system approaches to modify single species advice such as in the second tier approach of ICNAF. For example total surplus production model could be used to set system limits, or perhaps a trophic dynamic approach.

Evans: As an aside, could we agree not to call that holistic. For example a 10 species surplus production model is not holistic, it is a reductionist multispecies model.

I think that the most appropriate approach is to start simple and increment the size of the model as there is a need, and a possibility, as we have discussed.

O'Boyle: Furthermore, the added second tier, i.e. further regulation, will not be easy to implement. Ideally, in going toward a multispecies approach one would do away with the first tier. However, we have no idea as yet what kind of behaviour, e.g. oscillations, that would produce in the system. There is a great deal more that must be done before we even consider that level of approach.

Chairman: That appears to conclude the discussion, provided there are no further comments.

The following recommendations have been made:

1) That in general MEES function as a forum for peer review of information which could be of potential importance in modifying scientific advice to management. To achieve this MEES should hold an annual meeting with, in each year, a few main focal areas in which progress could be reviewed.

2) That a working group be formed to consider the practicality of using some of the available methods of modelling mixed fisheries (technological interactions) to modify single species advice. This working group would report to MEES at the next annual meeting.

3) That a few specific small systems of potential management importance be identified and that progress in these be reviewed annually until the results can be incorporated into the scientific advice. Specifically the cod-capelin system and the silver hake-haddock system were identified as requiring closer attention.

4) That the subcommittee review the potentially important area of the influence of environment on recruitment and distribution. More specifically a) recent work on broad scale environmental patterns and their possible influence on recruitment, and b) past models by Sutcliffe and colleagues and how well they have performed in retrospect.